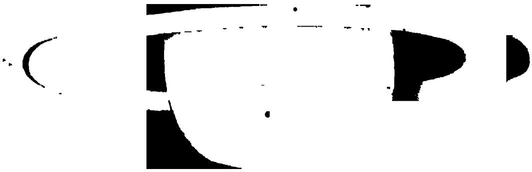
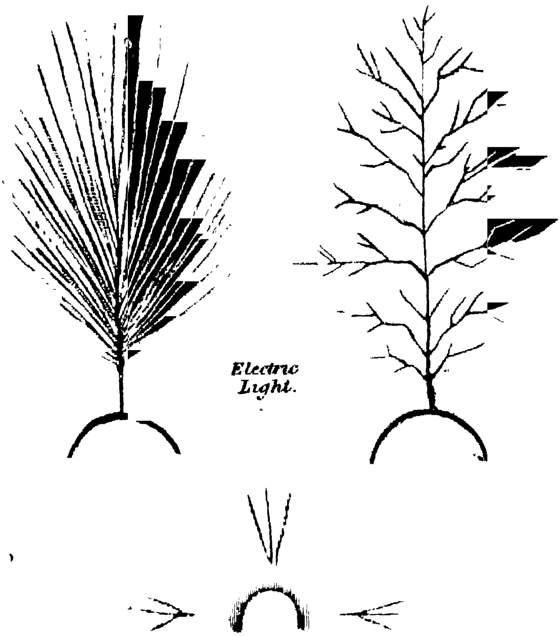
Singular Figure of Saturn





Electric Light.

A

JOURNAL

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CHEMISTRY,

AND

THE ARTS.

VOL. XIII.

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BY WILLIAM NICHOLSON.

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PREFACE.

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Soho Square, London, May 1, 1806.

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SUPPLEMENT.

TO THE BINDER.

The three large folding Plates numbered *Plate* 1, *Pl.* 2, and *Pl.* 3, *engraved from Trans. R. S.* are to be placed along with Plate 13. There are no Plates numbered 9, 10, 11, 12, these three Quartos supplying their Place.

A

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JANUARY, 1806.

ARTICLE I.

On the Cause of Fairy Rings. By JOHN GOUGH, *Esq.*

To Mr. NICHOLSON,

SIR, *Middleshaw, December* 7, 1805.

YOU published in the first page of your ninth volume in octavo, a letter to me from the Rev. Jonathan Wilson, vicar of Biddulph, in which the appearance of a patch of ground recently blasted and torn up by lightning was described. The observations of this ingenious and accurate gentleman promised to throw light on the natural history of fairy-rings, provided they were continued, and in this expectation, I took the liberty in a note subjoined to the copy of his letter printed by you, to request his future remarks on the subject, drawn from repeated inspection of the place affected by the lightning. The following is an extract of a letter from Mr. Wilson, containing his observations relative to the subsequent appearances of the patch, with some thoughts which are certainly an improvement in one theory of fairy-rings, that has received the patronage of some writers. This letter is dated November 1, 1805, and after some prefatory matter, proceeds thus, Introductory remarks.

Mr. Wilson's remarks begin. "In consequence of your esteemed favour of the 14th of August, 1804, I went on the 2nd of September following, to view the place which the lightning had struck, being accompanied by the farmer of the grounds. The affected spot was not then very easy to be distinguished, as the injured thistles were generally overtopped at the time, but we had no doubt of its true situation, upon finding the place where we formerly dug in pursuit of the imaginary stone. Some dead grass appeared, but it was confined to the space within which the roots had been plowed up by the electric fluid. The verdure was undoubtedly brighter about the hole, and the farmer was willing to attribute the flourishing state of this circle of herbitage to the lightning; but for my part, I ascribed it to what had dropped from his cows, rather than any thing that had fallen from the clouds.

The place not easy to be found.

Slight vestiges of the lightning.

These vestiges not permanent. "I have not been able to perceive the least difference between the part struck, and the rest of the field, during the course of the present year; my observations must therefore be acknowledged not to favour the hypothesis, which supposes fairy-rings to be formed at first by lightning.

The explanation by grubs improved. "I never saw a fairy-ring, and therefore may seem badly qualified to write on the subject; but from what I have read, it appears to me, that the circle of decayed grass is caused by the innumerable grubs, which are said to lay concealed under the ring among the roots of the herbitage; I also suppose, that the funguses commonly seen on fairy-rings, give a preference to these circles on account of the abundance of dead vegetable matter to be found in them; amongst which various species of fungi are known to grow. To this I may add, that the interior circle of dark green grass is owing to the dung, and ploughing of those animals in the preceding year; and the reason which compels these grubs or their offspring to push forward from the centre, seems to be this; every creature we know of has an aversion to working in its own excrement, or that of its own species."

Reflections on these observations. The observations of Mr. Wilson, stated above, seem to demonstrate, that a patch of herbage is not invariably converted into a fairy-ring by a powerful stroke of lightning; consequently if electric discharges from the atmosphere be the primary causes of these circles, they require the assistance of some peculiarity in the soil to give permanency to the appear-

ance,

ance. Moreover the circular figure of these phenomena embarrasses the electrical hypothesis with a second difficulty; for the tracts of discoloured grass, actually produced by lightning, are but seldom bent into rings, as they more frequently assume a zigzag, or else a ramified form, and are, I believe, of but short duration; which shews, the roots of the herbage are not destroyed, unless where the earth is torn up. The theory which attributes these circles of withered grass to the running of a fungus, has little or no foundation; because these imperfect plants, generally speaking, attach themselves to dead vegetables, consequently their presence in fairy-rings is nothing more than an appearance which is subsequent to the destruction of the herbage upon them. As for the lively verdure of the grass on the interior edges of these circles, I believe it may be explained upon general principles, without the agency of lightning or fungi. For if the herbage of a patch of ground be destroyed root and branch by a cause which does not remove the remains of it, the place will be covered, in process of time, with a fresh crop of plants, possessing superior luxuriancy and verdure. The causes of this vigorous vegetation appear to be the following; the dead roots and stems rot and manure the soil which produced them: this source of fertility receives an additional supply from a succession of fungi, which grow and decay on the surface of the ground, as well as from the excrement and exuvia of the grubs, which take up their abode among the withered roots; lastly, the soil is rendered more porous by the decay of the vegetable remains, and thereby becomes more permeable to the air, which increases its fertility not a little. The last position seems to be confirmed by the circumstance of plants thriving better in unglazed, than in glazed flower pots. The following facts may be adduced in corroboration of what has been here advanced. A small piece of ground was covered, in June, with common salt, which had been rubbed upon the corps of a drowned man; the herbage of this place died in a short time; but was succeeded the next summer by a new crop, the verdure of which distinguished it for some years from the surrounding grass. As common salt is esteemed a manure, perhaps the following instance will be called preferable to the former. Many woods in this country, especially those about Windermere, are cut

Difficulties of the electrical theory.

Fungi not the cause of fairy rings.

Superior verdure of these rings explained.

Dead plants, &c. manure the ground.

The air increases vegetation by acting on the roots.

Proofs of the preceding theory.

down about once in fourteen years, and converted into charcoal, for the use of the iron works. This is done by throwing the branches into large heaps, which are then covered with green turf, and set on fire. These piles of wood continue burning for several days, in consequence of which, the roots in the ground beneath them are completely charred; and the pit-stead, as the place is called, has no verdure left upon it. The loss however is repaired in the course of a few years by nature herself, where art does not interfere, and the spot is clothed with a fresh coat of herbage, consisting of herbs remarkable for their size and flourishing appearance. This instance of vigorous vegetation on ground that has been completely burned, in all probability, is occasioned by the texture of the soil; which is adapted to retain moisture, and admit the air; unless we suppose the incorruptible substance of charcoal to afford a species of manure.

The preceding hints may perhaps incite some of your readers to study the natural history of fairy-rings with greater diligence, especially to search after the true cause which blasts these circles; for when this is discovered, we shall be able to re-clothe them with superior verdure, on rational principles.

I remain, &c.

JOHN GOUGH.

II.

Observations on the singular Figure of the Planet Saturn. By WILLIAM HERSCHEL, *LL. D. F. R. S.**

Examination of the striking phenomena of the planet Saturn.

THERE is not perhaps another object in the heavens that presents us with such a variety of extraordinary phenomena as the planet Saturn: a magnificent globe, encompassed by a stupendous double ring: attended by seven satellites: ornamented with equatorial belts: compressed at the poles: turning upon its axis: mutually eclipsing its ring and satellites, and eclipsed by them: the most distant of the rings also turning upon its axis, and the same taking place with the farthest of the satellites: all the parts of the system of Saturn occasionally

* Philosophical Transactions, 1805.

reflecting

reflecting light to each other: the rings and moons illuminating the nights of the Saturnian: the globe and satellites enlightening the dark parts of the rings: and the planet and rings throwing back the sun's beams upon the moons, when they are deprived of them at the time of their conjunctions.

It must be confessed that a detail of circumstances like these, appears to leave hardly any room for addition, and yet the following observations will prove that there is a singularity left, which distinguishes the figure of Saturn from that of all the other planets.

It has already been mentioned on a former occasion, that so far back as the year 1776 I perceived that the body of Saturn was not exactly round; and when I found in the year 1781 that it was flattened at the poles at least as much as Jupiter, I was insensibly diverted from a more critical attention to the rest of the figure. Prepossessed with its being spheroidical, I measured the equatorial and polar diameters in the year 1789, and supposed there could be no other particularity to remark in the figure of the planet. When I perceived a certain irregularity in other parts of the body, it was generally ascribed to the interference of the ring, which prevents a complete view of its whole contour; and in this error I might still have remained, had not a late examination of the powers of my ten-feet telescope convinced me that I ought to rely with the greatest confidence upon the truth of its representations of the most minute objects I inspected.

Its body oblate.

Reasons why the greater peculiarities of its figure were overlooked:

The following observations, in which the singular figure of Saturn is fully investigated, contain many remarks on the rest of the appearances that may be seen when this beautiful planet is examined with attention; and though they are not immediately necessary to my present subject, I thought it right to retain them, as they show the degree of distinctness and precision of the action of the telescope, and the clearness of the atmosphere at the time of observation.

April 12, 1805. With a new 7-feet mirror of extraordinary distinctness, I examined the planet Saturn. The ring reflects more light than the body, and with a power of 570 the colour of the body becomes yellowish, while that of the ring remains more white. This gives us an opportunity to distinguish the ring

Very perfect observation, in which the circumference was seen to be flattened in four regions.

ring from the body, in that part where it crosses the disk, by means of the difference in the colour of the reflected light. I saw the quintuple belt, and the flattening of the body at the polar regions; I could also perceive the vacant space between the two rings.

Observations on Saturn by which its singular figure is ascertained.

The flattening of the polar regions is not in that gradual manner as with Jupiter, it seems not to begin till at a high latitude, and there to be more sudden than it is towards the poles of Jupiter. I have often made the same observation before, but do not remember to have recorded it any where.

April 18; ten-feet reflector, power 300. The air is very favourable, and I see the planet extremely well defined. The shadow of the ring is very black in its extent over the disk south of the ring, where I see it all the way with great distinctness.

The usual belts are on the body of Saturn; they cover a much larger zone than the belts on Jupiter generally take up, as may be seen in the figure I have given in Plate I.; and also in a former representation of the same belts in 1794.*

The figure of the body of Saturn, as I see it at present, is certainly different from the spheroidical figure of Jupiter. The curvature is greatest in a high latitude.

I took a measure of the situation of the four points of the greatest curvature, with my angular micrometer, and power 527. When the cross of the micrometer passed through all the four points, the angle which gives the double latitude of two of the points, one being north the other south of the ring, or equator, was 93° 16′. The latitude therefore of the four points is 46° 38′; it is there the greatest curvature takes place. As neither of the cross wires can be in the parallel, it makes the measure so difficult to take, that very great accuracy cannot be expected.

The most northern belt comes up to the place where the ring of Saturn passes behind the body, but the belt is bent in a contrary direction being concave to the north, on account of its crossing the body on the side turned towards us, and the north pole being in view.

There is a very dark, but narrow shadow of the body upon the following part of the ring, which as it were cuts off the ring from the body.

* See Phil. Transf. for 1794, Table VI. page 32.

The

The ſhadow of the ring on the body, which I ſee ſouth of the ring, grows a little broader on both ſides near the margin of the diſk.

Obſervations on Saturn, by which its ſingular figure is aſcertained.

The diviſion between the two rings is dark, like the vacant ſpace, between the anſæ, but not black like the ſhadow I have deſcribed.

There are four ſatellites on the preceding ſide near the ring; the largeſt and another are north-preceding; the other two are nearly preceding.

April 19. I viewed the planet Saturn with a new 7-feet telescope, both mirrors of which are very perfect. I ſaw all the phenomena as deſcribed laſt night, except the ſatellites, which had changed their ſituation; four of them being on the following ſide. This teleſcope however is not equal to the 10-feet one.

The remarkable figure of Saturn admits of no doubt: when our particular attention is once drawn to an object, we ſee things at firſt ſight that would otherwiſe have eſcaped our notice.

10-feet reflector, power 400. The night is beautifully clear, and the planet near the meridian. The figure of Saturn is ſomewhat like a ſquare or rather parellelogram, with the four corners rounded off deeply, but not ſo much as to bring it to a ſpheroid. I ſee it in perfection.

The four ſatellites that were laſt night on the preceding, are now on the following ſide, and are very bright.

I took a meaſure of the poſition of the four points of the greateſt curvature, and found it 91° 29′. This gives their latitude 45° 44′,5. I believe this meaſure to be pretty accurate. I ſet firſt the fixed thread to one of the lines, by keeping the north-preceding and ſouth-following two points in the thread; then adjuſted the other thread in the ſame manner to the ſouth-preceding and north-following points.

May 5, 1805. I directed my 20-feet teleſcope to Saturn, and, with a power of about 300, ſaw the planet perfectly well defined, the evening being remarkably clear. The ſhadow of the ring on the body is quite black. All the other phenomena are very diſtinct.

The figure of the planet is certainly not ſpheroidical, like that of Mars and Jupiter. The curvature is leſs on the equator and

Observations on Saturn, by which its singular figure is ascertained.

and on the poles than at the latitude of about 45 degrees. The equatorial diameter is however confiderably greater than the polar.

In order to have the teftimony of all my inftruments, on the fubject of the ftructure of the planet Saturn, I had prepared the 40-feet reflector for obferving it in the meridian. I ufed a magnifying power of 360, and faw its form exactly as I had feen it in the 10 and 20-feet inftruments. The planet is flattened at the poles, but the fpheroid that would arife from this flattening is modified by fome other caufe, which I fuppofe to be the attraction of the ring. It refembles a parallelogram, one fide whereof is the equatorial, the other the polar diameter, with the four corners rounded off fo as to leave both the equatorial and polar regions flatter than they would be in the regular fpheroidical figure.

The planet Jupiter being by this time got up to a confiderable altitude, I viewed it alternately with Saturn in the 10-feet reflector, with a power of 500. The outlines of the figure of Saturn are as defcribed in the obfervation of the 40-feet telefcope; but thofe of Jupiter are fuch as to give a greater curvature both to the polar and equatorial regions than takes place at the poles or equator of Saturn which are comparatively much flatter.

May 12. I viewed Saturn and Jupiter alternately with my large 10-feet telefcope of 24 inches aperture; and faw plainly that the former planet differs much in figure from the latter.

The temperature of the air is fo changeable that no large mirror can act well.

May 13. 10-feet reflector, power 300. The fhadow of the ring upon the body, and of the body upon the ring, are very black, and not of the dufky colour of the heavens about the planet, or of the fpace betwen the ring and planet, and between the two rings. The north-following part of the ring, clofe to the planet, is as it were cut off by the fhadow of the body; and the fhadow of the ring lies fouth of it, but clofe to the projection of the ring.

The planet is of the form defcribed in the obfervation of the 40-feet telefcope; I fee it fo diftinctly that there can be no doubt of it. By the appearance, I fhould think the

points

points of the greateſt curvature not to be ſo far north as 45 degrees.

Obſervations on Saturn, by which its ſingular figure is aſcertained.

The evening being very calm and clear, I took a meaſure of their ſituation, which gives the latitude of the greateſt curvature 45° 21′. A ſecond meaſure gives 45° 41′

Jupiter being now at a conſiderable altitude, I have viewed it alternately with Saturn. The figure of the two planets is decidedly different. The flattening at the poles and on the equator of Saturn is much greater than it is on Jupiter, but the curvature at the latitude of from 40 to 48° on Jupiter is leſs than on Saturn.

I repeated theſe alternate obſervations many times, and the oftener I compared the two planets together, the more ſtriking was their different ſtructure.

May 26. 10-feet reflector. With a parallel thread micrometer and a magnifying power of 400, I took two meaſures of the diameter of the points of greateſt curvature. A mean of them gave 64,3 diviſions = 11″,98. After this, I took alſo two meaſures of the equatorial diameter, and a mean of them gave 60,5 diviſions = 11″,27; but the equatorial meaſures are probably too ſmall.

To judge by a view of the planet, I ſhould ſuppoſe the latitude of the greateſt curvature to be leſs than 45 degrees. The eye will alſo diſtinguiſh the difference in the three diameters of Saturn. That which paſſes through the points of the greateſt curvature is the largeſt; the equatorial the next, and the polar diameter is the ſmalleſt.

May 27. The evening being very favourable, I took again two meaſures of the diameter between the points of greateſt curvature, a mean of which was 63,8 diviſions = 11″,68. Two meaſures of the equatorial diameter gave 61,3 diviſions = 11″,44.

June 1. It occurred to me that a more accurate meaſure might be had of the latitude in which the greateſt curvature takes place, by ſetting the fixed thread of the micrometer to the direction of the ring of Saturn, which may be done with great accuracy. The two following meaſures were taken in this manner, and are more ſatisfactory than I had taken before. The firſt gave the latitude of the ſouth-preceding point of greateſt curvature 43° 26′; and the ſecond 43° 13′. A mean of the two will be 43° 20′.

June

Obfervations on Saturn, by which its fingular figure is afcertained.

June 2. I viewed Jupiter and Saturn alternately with a magnifying power of only 300, that the convexity of the eye-glafs might occafion no deception, and found the form of the two planets to differ in the manner that has been defcribed.

With 200 I faw the difference very plainly; and even with 160 it was fufficiently vifible to admit of no doubt. Thefe low powers fhow the figure of the planets perfectly well, for as the field of view is enlarged, and the motion of the objects in paffing is leffened, we are more at liberty to fix our attention upon them.

I compared the telefcopic appearance of Saturn with a figure drawn by the meafures I have taken, combined with the proportion between the equatorial and polar diameters determined in the year 1789;* and found that, in order to be a perfect refemblance, my figure required fome fmall reduction of the longeft diameter, fo as to bring it nearly to agree with the meafures taken the 27th of May. When I had made the neceffary alteration, my artificial Saturn was again compared with the telefcopic reprefentation of the planet, and I was then fatisfied that it had all the correctnefs of which a judgment of the eye is capable. An exact copy of it is given in Plate IX. The dimenfions of it in proportional parts are,

The diameter of the greateft curvature -	36
The equatorial diameter - - - - -	35
The polar diameter - - - - - -	32
Latitude of the longeft diameter -	43° 20.′

The foregoing obfervations of the figure of the body of Saturn will lead to fome intricate refearches, by which the quantity of matter in the ring, and its folidity, may be in fome meafure afcertained. They alfo afford a new inftance of the effect of gravitation on the figure of planets; for in the cafe of Saturn, we fhall have to confider the oppofite influence of two centripetal and two centrifugal forces: the rotation of both the ring and planet having been afcertained in fome of my former Papers.

* See Phil. Tranf. for 1790, page. 17.

III.

Facts and Observations on the medical Respiration of gazeous Oxide of Azote. In a Letter from Dr. BEDDOES.

To Mr. NICHOLSON.

SIR,

DR. Pfaff's paper on respiration* will probably draw the attention of the scientific towards the gaseous oxide of azote, which has been too much neglected in a medical point of view. I was only sorry to see that he proposes to use it in melancholia. No combination of ideas can be more obvious than the application of an agent which has so frequently proved exhilarating, and never yet been observed to be followed by exhaustion where it did exhilarate, to a complaint, in which depression of spirits is a striking circumstance. But I am apprehensive that the first thoughts of inexperience here (as so often happen) will prove illusory, and that this project will not be followed by the expected advantage in many cases of melancholia. For if it be true that there is no real distinction between mania and melancholia, as far as the sensorium is concerned, and that the vivacity of ideas in melancholia answers to the violence of muscular actions in mania, as I have endeavoured to shew in my *Essays on Health*; is there not ground to apprehend that the actions of the brain, already too strong, will be increased by this gas, or the diseased contemplations rendered more intense?

Dr. Pfaff's experiments on respiration.

Proposes gaseous oxide of azote in melancholy madness.

If there be any state of melancholia in which it may be of service, this will probably happen when the nervous system is falling into debility, in consequence of having been kept too much on the stretch.

Cautions against its unguarded use, from theory;

But I do not here warn against gaseous oxide from mere theory. The manager of a lunatic asylum near Bristol, respectably known to the public, concurred with me some years ago in the opinion which I expressed to him concerning its probable advantage in melancholia; and a patient that had been under his care inhaled it fairly without benefit. The administration was tried in two other cases as fruitlessly: Indeed

from experience.

Cases.

* Philos. Journal, XII, 249.

I dis-

I discontinued it in one, from some indications of an aggravation of the symptoms. I was by this time alive to suspicion, having thought much on the subject, and reasoned myself into the idea that it would often do injury upon the above-mentioned principle. It has long been my opinion, and there are striking observations on record to prove that hidrogen, hidro-carbonate, azotic, or carbonic acid gases, would be more likely to answer in active insanity under whatever form. These observations I shall take occasion to quote hereafter.

Use in palsy of one kind.

The very first time I witnessed the effects of gaseous oxide on a person in health, I concluded, that it would be a remedy in certain cases of palsy. A patient who had emerged from apoplexy with the loss of the power of one side of his body, was accordingly put under a course of the gas. The result completely answered expectation. The case was most carefully watched; and on withholding the gas, the symptoms repeatedly grew worse, and *vice versa*. After the patient's recovery, he was kept under inspection for a considerable time, and did not relapse. This has been confirmed by other results; and in palsy, where the brain is primarily affected, I expect that Dr. Pfaff will find either a cure or great relief to follow the use of this gas in a respectable proportion of cases.

In another kind of palsy.

I have very fairly tried it in palsy apparently from cold, beginning at the extremities and creeping from muscle to muscle, without good or bad effect. There is a case of this kind, related by Dr. Kentish, with the patient's name, and corroborated by testimony superior to all exception in *Considerations on factitious Airs (Johnson)* in which a perfect cure was obtained from oxigen gas; and I have since learned, by experiments carefully repeated before various philosophical observers, that *in essential respects, oxigen gas and gaseous oxide act in a very different, nay opposite manner upon the living fibre.*

Or oxigen,

These experiments I hope to publish before midsummer.

From palsy, analogy led me to other cases of debility. I fully tried gaseous oxide in dropsy of the chest (anasarca of the lungs), but without good or bad effect. I was much disappointed, conceiving that in dropsy (at least in one species) we have a paralytic state of the lymphatics. But I have been since assured by a physician, that for some dropsies he has found

In dropsies;

found a remedy in this gas. There are dropſies which doubtleſs depend on exceſs of exhalant action. Theſe are eaſily diſtinguiſhed; and they require bleeding as much as pleuriſy.

In debility, ariſing from reſidence in hot climates and from intenſe application to buſineſs, I have known gaſeous oxide completely ſucceſsful after an infinity of remedies, Bath and other waters, had been tried in vain. In other ſtates of debility.

The particulars of theſe caſes are alſo deſtined for publication: But I reſolved to wait for ſome years after the uſe of the gas; for I have found that a ſingle circumſtance vitiates a large proportion of our medical records. Patients after an apparent recovery fall again into the ſame complaint; and there are other conſiderations, which I ſhall for the preſent paſs over.

If Mr. Pfaff uſes gaſeous oxide in palſy, he will probably ſooner or later ſee a phenomenon as extraordinary as any in galvaniſm, and which after it has been deſcribed by a philoſopher of high reputation, will become equally celebrated. This is the inſtantaneous reſtoration of voluntary power over a limb deprived both of motion and feeling by palſy ſucceeding to apoplexy, while the patient is reſpiring gaſeous oxide. This was witneſſed in common with myſelf, by ſeveral reſpectable perſons; and among others by ſome of your philoſophical acquaintance, if I do not miſtake. It was in the caſe of Mr. G. a member of the laſt parliament, who completely recovered: But as other means were afterwards adopted, I do not impute the reſult to the gas, which however, when uſed alone, was viſibly of great ſervice: for I have no idea of claiming for a remedy under ſcrutiny any cure, if other powers have been called in at the ſame time. Gaſeous oxide has given voluntary power over palſied parts, while inhaled.

I tranſmit theſe obſervations to you, Sir, in preference to the Editor of any Medical Journal, becauſe I think them likely to meet the eye of Dr. Pfaff ſooner in your Journal. I ſhould be extremely ſorry that he ſhould ſet out wrong in his trials, becauſe the fault will be imputed to the power itſelf, and not to its miſapplication; and the diſabled will ſtill be left to languiſh and be cut off, notwithſtanding we have a remedy at hand. Firſt wiſhed for effect of this ſtatement.

I have another reaſon. I moſt ſincerely wiſh any thing I could ſay would haſten the period, which *muſt* arrive, when medical ſcience ſhall not be merely what the Germans call a A ſecond.

Brod-

Brod-wissenschaft, or pursued only for a livelihood. If philosophical men without a profession would take it up, it is I think certain, that it must soon become both more efficient and more liberal. Any study is capable of interesting the feelings; and most surely that of the laws of the organic world is as much so as any other. Opportunities of anatomical, chemical, and *clinical* information are at hand. A person so prepared will, heaven knows, with ardour and industry soon acquire all that is useful in medical practice. Let him then, animated with no other motive than the pure desire of benefiting his fellow men, apply himself to the improvement of medicine. It is impossible that he should not succeed as fully as our Tennants, our Hatchetts, and Chenevix's have done in chemistry; for it is not its inherent difficulty, but collateral circumstances, that retard the progress of this art. Many apothecaries, for example, and old women in general, who are the great controulers of the destiny of physicians, would by no means allow the use of gaseous oxide in palsy, though the patient in the course both of nature and of ordinary medication be sure to die, and perhaps in a very miserable manner. But the philosophical cultivator of medicine, without troubling himself about the good opinion of the one or the other, would proceed on his career under the guidance of the collective light of science and of humanity.

Men of liberal curiosity exhorted to study medicine.

Anecdote.

N. N. advanced in years, of a thickset stature, and with a short neck, shewed signs of palsy many years ago. The writer of these lines warned his friends of the danger. Concurring in this apprehension, Dr. Ingenhousz proposed to him to inhale oxigen gas, a practice familiar to that accurate philosopher, and by which he hoped the constitution might be recruited. The execution of the idea was deferred. Meanwhile the gaseous oxide was discovered to be respirable, and its power in palsy was to a degree ascertained. The writer now pressed the use of this gas with the utmost earnestness. The patient saw it taken by others: He himself consented to inhale it, when behold! the distress of a lady present, as excited by some apprehended imaginary bad consequences, put off the inhalation. The predicted paralytic seizure arrived: but there was ample time still for the use of the oxide. I proposed that another patient, situated as similarly as possible, should be sought; and that if he consented upon the

credit

credit of the ſucceſsful exhibition, and upon my reſponſibility, to uſe the gas, the reſult ſhould determine as to its employment in the caſe firſt in queſtion. At the ſame time, I ſtated from the average courſe of paralytic attacks in general not immediately fatal, that a little apparent amendment would take place, and the ſtroke return with additional violence. My propoſal was acknowledged to be highly reaſonable; but that plan of routine treatment was followed which is ſo much more advantageous to the idle and unſcientific of our profeſſion than it is to the ſick, and the patient died of a return of his complaint. Such is probably the condition of thouſands of the diſeaſed at this moment! Rather than uſe a recently propoſed plan not in the Pharmacopœia, or ſeek a new one in analogy, we perſevere in painful or diſguſting means, from which, on the faith of long experience, no good of any ſort can be expected for the ſufferer. May the riſing generation of natural philoſophers exerciſe their talents and their benevolence in putting an end to ſo crying an evil!

I am, Dear Sir,

Reſpectfully your's,

THOMAS BEDDOES.

Clifton, Dec. 13, 1805.

P. S. A caſe in your Journal, where a gentleman accuſtomed to breathe gaſeous oxide for amuſement, experienced very diſagreeable feelings on one particular occaſion, ſeems to me clearly referable to hyſteria. Now the trials at the Pneumatic Inſtitution, as related in Mr. Davy's *Reſearches*, had clearly ſhewn that in the prediſpoſed, gaſeous oxide is a ſpecific for exciting an hyſteric paroxyſm. Perhaps in the individual whoſe caſe is related by himſelf in the Journal, no obvious prediſpoſition, either temporary or permanent, exiſted: Nothing to this purport is ſtated. But that the affection was ſimply hyſterical cannot I think be doubted by any one converſant both with hyſteria and the adminiſtration of gaſeous oxide. It ſeems to be ſtrongly marked by that idea of immediate danger, which is ſo common in hyſteria. Dr. Garnet very unneceſſarily, and, I believe, very miſtakenly, called up the whole Brunonian theory on the emergency. It led him, however, to give cordials; and they were proper. A tea-ſpoonful of ſal volatile, from

On effects of gas. ox. as ſtated in this Journ.

Their real nature.

from time to time, would probably have anfwered without the Brunonian theory. But it is certainly the bufinefs of the phyfician to avoid gafeous oxide in the hyfterical, as it is wine in thofe who labour under acute inflammation. If your correfpondent who related his own feelings could fpecify any caufe which might have rendered him nervous, or ftate the fact whether he was fo or not, it would give fatisfaction to the prefent writer, and perhaps alfo to future inquirers.

Caution regarding particular fubjects. Quære.

To interdict a remedy becaufe its ufe requires difcrimination, would, in many diforders, be leaving the fick to certain deftruction. I imagine that the outcry againft fuch means as gafeous oxide, will arife from thofe who daily ufe the moft hazardous remedies, and who are enabled to do it without reproach, becaufe they are put into a phial, and the patient and his friends never trouble themfelves about the nature of the articles which they are receiving into the ftomach.

Remark on dangerous diforders.

IV.

Abftract of Obfervations on a diurnal Variation of the Barometer between the Tropics. By J. HORSBURGH, *Efq. In a Letter to* HENRY CAVENDISH, *Efq. F. R. S.** *Read March* 14, 1805.

SIR, *Bombay, April*, 20, 1804.

Tropical variation of the barometer.

WHEN I was in London at the conclufion of the year 1801, I had the pleafure of being introduced to you by my friend Mr. Dalrymple, at which time he prefented you with fome fheets of meteorological obfervations, with barometer and thermometer, made by me in India, and during a paffage from India to England.

Being of opinion that few regifters of the barometer are kept at fea, efpecially in low latitudes, I have been induced to continue my obfervations fince I left England, judging that, even if they were found to be of no utility, they might at leaft be entertaining to you or other gentlemen, who have been making obfervations of a fimilar nature.

During my laft voyage I have employed two marine barometers, one made by Troughton, the other by Ramfden,

* Philofophical Tranfactions, 1805.

and

and a thermometer by Frazer. These were placed exposed to a free current of air in a cabin, where the basons of the barometers were 13 feet above the level of the sea. Tropical variation of the barometer.

The hours at which the heights of the barometers, and thermometers were taken, *viz.* noon, 4 hours, 10 hours, 12 hours, 14 hours, and 19 hours, were chosen, because at these times the mercury in the barometer had been perceived to be regularly stationary between the tropics, by former observations made in India in 1800 and 1801. It was found that in settled weather in the Indian seas, from 8 A M to noon, the mercury in the barometer was generally stationary, and at the point of greatest elevation; after noon it began to fall, and continued falling till 4 afternoon, at which time it arrived at the lowest point of depression. From 4 or 5 P M the mercury rose again, and continued rising till about 9 or 10 P M, at which time it had again acquired its greatest point of elevation, and continued stationary nearly till midnight; after which it began to fall, till at 4 A M it was again as low as it had been at 4 afternoon preceding; but from this time it rose till 7 or 8 o'clock, when it reached the highest point of elevation, and continued stationary till noon.

Thus was the mercury observed to be subject to a regular elevation and depression twice in every 24 hours in settled weather; and the lowest station was observed to be at about 4 o'clock in the morning and evening. I remarked that the mercury never remained long fixed at this low station, but had a regular tendency to rise from thence till towards 8 in the morning and about 9 in the evening, and from those times continued stationary till noon and midnight.

In unsettled blowing weather, especially at Bombay during the rains, these regular ebbings and flowings of the mercury could not be perceived; but a tendency to them was at some times observable when the weather was more settled.

In the sheets, which I formerly presented to you, were evinced these elevations and depressions twice every 24 hours within the tropics, in steady weather, as had been observed by Mess. Cassan and Peyrouse, by Dr. Balfour of Calcutta, and others. But since my last arrival in India, I have observed that the atmosphere appears to produce a different effect on the barometer at *sea* from what it does on *shore*.

 As

Tropical variation of the barometer.

As I am ignorant whether this phenomenon has been noticed by any perſon before, I will here give you an abſtract of my journal, ſhewing how the barometer has been influenced during the whole time ſince I left England, which will enable you to form an idea whether I am right in concluding that the barometer is really differently affected at ſea from what it is on ſhore, at thoſe places in India where the obſervations have been made.

The firſt ſheet begins with the obſervations made on board ſhip, in my voyage from London towards Bombay, in the months of April and May, 1802.

From the time of leaving the Land's End, April 19th, the motion of the mercury in barometers was fluctuating and irregular until we were in latitude 26° N, longitude 20° W, on April 29th; the mercury in barometers then became uniform in performing two elevations and two depreſſions every 24 hours, (which for brevity in mentioning hereafter I will call equatropical motions.) From latitude 26° N to latitude 10° N, the difference of the high and low ſtations of the mercury in the barometers was not ſo great, as it was from latitude 10° N acroſs the equator, and from thence to latitude 25° S. Within theſe laſt-mentioned limits, the difference of high and low ſtations of the mercury in the barometers was very conſiderable, generally from five to nine hundred parts of an inch, both in the daily and nightly motions.

When we reached the latitude of 28° S, longitude 27° W, June 7th, the mercury in barometers no longer adhered to the equatropical motions; but then, as in high north latitudes, its riſing and falling became irregular and fluctuating during our run from latitude 28° S, longitude 27° W, (moſtly between the parallels of 35° and 36° S,) until we were in latitude 27° S, and longitude 51° E, on the 11th of July. The mercury then began to perform the equatropical motions, and continued them uniformly, during our run from the laſt-mentioned poſition, up the Madagaſcar Archipelago, acroſs the Equator, until our arrival at Bombay July 31ſt. 1802.

Auguſt 6th, 1802. When the barometers were placed on ſhore in Bombay, the mercury, for the firſt ſix days, appeared to have a ſmall tendency towards performing the equatropical motions, but not equally perceptible as when at ſea, the difference between the high and low ſtations of the mercury in

the barometers being great to the day we entered the harbour of Bombay. From the 12th of Auguſt to the 22d the mercury could not in general be obſerved to have any inclination to perform the equatropical motions, although at times a very ſmall tendency towards performing them might be perceived.

Tropical variation of the barometer.

On the 23d of Auguſt the barometers were taken from the ſhore to the ſhip. Immediately on leaving Bombay harbour, Auguſt 26th, 1802, the mercury in the barometers performed the equatropical motions, and continued them with great uniformity, during our paſſage down the Malabar coaſt, acroſs the bay of Bengal, in the Strait of Malacca, and through the China Sea, until our arrival in Canton river on the 4th of October. When in the river, the mercury became nearly ſtationary during the 24 hours, except a ſmall inclination at times towards the equatropical motions, but they were not near ſo perceptible as at ſea; this change taking place the day we got into the river.

During our ſtay in China, the barometer on ſhore, at Canton, had very little tendency towards the equatropical motions, throughout the months of October and November that we remained there. At times, while in China, a ſmall inclination towards performing the equatropical motions appeared: but, as in Bombay, the difference of riſe and fall was of ſo ſmall a quantity, as to be frequently imperceptible.

December 2d, 1802. On our departure from Canton river, the equatropical motions were inſtantly performed by the mercury, and with great regularity continued during the whole of the paſſage to Bombay, until our arrival in that harbour on the 11th of January, 1803.

On January 18th, the barometers were placed on ſhore, and did not appear in the ſmalleſt degree ſubject to the equatropical motions; although, with great regularity, they had been performed while at ſea, even to the day we entered the harbour. One of the barometers was left on board for a few days, and, like that on ſhore, ſeemed to have no tendency towards the equatropical motions. During the months of February and March, in Bombay, the mercury was nearly ſtationary throughout the 24 hours. But about the latter part of March the mercury ſeemed to incline towards the equatropical mo-

Tropical variation of the barometer.

tions in a very fmall degree; and, during the month of April, and to the 20th of May, this fmall tendency of the mercury to perform the motions appeared at times, but was hardly difcernible, the rife and fall being of fo fmall a quantity. From the 18th of January to the 20th of May, the mercury in the barometers was in general ftationary, except a very fmall tendency towards the equatropical motions at times. At other times fome change in the atmofphere difturbed the mercury from its ftationary pofition; but this was feldom the cafe, as it was then the fair weather feafon, or north-eaft monfoon.

We failed from Bombay on the 23d of May, 1803. The inftant we got out of the harbour, the mercury in the barometers conformed to the equatropical motions with great regularity, and the difference between the high and low ftations was very confiderable during the whole of the paffage to China, excepting a few days in the eaftern parts of Malacca Strait, where the land lay contiguous on each fide of us; the difference between the high and low ftations of the mercury was not then fo great as in the open fea. On clearing the Strait, and entering the China Sea, the equatropical motions were performed in greater quantity, and continued regular during our paffage up the China Sea, until July 2, 1803. We then entered Canton river, and the equatropical motions of the mercury in barometers entirely ceafed.

From July 8th to September 7th, the barometers were placed on fhore in Canton, during which time the mercury appeared to have no tendency towards performing the equatropical motions; but it inclined to a ftationary pofition, except when influenced by changes of weather. After the barometers were taken from Canton to the fhip; we were four days in getting clear of the river, in which time the mercury inclined to be ftationary, excepting that a fmall inclination towards the equatropical motions feemed to evince itfelf at times. But no fooner had we cleared Canton river, September 13, 1803, than the mercury in the barometers began to conform to the equatropical motions, of two elevations and two depreffions every 24 hours, at equal intervals of time, (although we were near the land until the 15th September.) And the mercury, with great regularity, continued to perform

form the equatropical motions, from September 13, 1803, the day we cleared the river of Canton, until October 13, when we entered Sincapore Strait, excepting a small degree of irregularity, which affected the mercury on the 22d September, when it blew a gale on the coast of Isiompa.

Tropical variation of the barometer.

October 13, 1803. On entering the Strait of Sincapore, which is about $3\frac{1}{2}$ leagues wide, the mercury in the barometers was then a little obstructed, and did not perform the equatropical motions, in the same quantity of rise and fall, as when we were in the China Sea. But on the following day, October 14, when we had passed the narrow part of the Strait, the mercury conformed to those motions with regularity until October 21, when we arrived in the harbour of Prince of Wales's Island; then a great retardation took place in the equatropical motions; for, during the time the ship remained in the harbour, from October 20 to November 5, 1803, the mercury in barometers seemed only in a small degree subject to them, the difference between the high and low stations of the mercury, being in general not more than half the quantity, that takes place in the open sea, or at a considerable distance from land. Where the ship lay at this time in the harbour, the land, on one side, was a full quarter of a mile distant, and on the other side about $1\frac{1}{2}$ mile.

On November 5, being clear of the harbour of Prince of Wales's Island, the equatropical motions were instantly performed by the mercury, in the usual quantity experienced at sea, which continued with uniformity until December 3. On this and the following day, the mercury fell considerably during our passage over the tails of the sands at the entrance of Hoogly river, in latitude 21° 06′ N; and on December 5, the day of the moon's last quarter, a gale of wind commenced from N N E, with much lightning and rain in the night. During the latter part of this day, the mercury began to rise, and there soon followed a change of settled weather. When we were in the lower part of the river, the mercury appeared to conform in a small degree to the equatropical motions; but when well up the river, at Diamond Harbour, the mercury inclined to be nearly stationary during the 24 hours, as has formerly been observed to happen in Canton river, Bombay harbour, &c.

On

Tropical variation of the barometer.

On January 13, 1804, after we had cleared the river Hoogly, the mercury in the barometers began to perform its motions with uniformity, which continued during the passage to Bombay, until our arrival there on February 12. The barometers being then placed on shore, the mercury inclined to a stationary position, without evincing any propensity towards the equatropical motions from the 12th to the 18th February, 1804, as has been noticed in the foregoing description, to happen frequently, on entering a harbour from sea.

On February 18, 1804, the meteorological journal ceases, at which time it comprises the observations of 22 months, having commenced April 6, 1802, in Margate Road.

I have taken the liberty of sending you this abstract from the journal, to exhibit the apparent difference of the mercury in the barometer at sea, from what has been observed on shore, at those places mentioned in the preceding description. As I have not seen any account indicating the phenomenon, I thought it might be interesting to you, or other gentlemen of the Royal Society to forward this imperfect abstract, the journal itself being too cumbersome to send home at present. But as I am in expectation of returning to England by the ships from China next season, I hope I shall be enabled to present you with the meteorological sheets alluded to.

I am, &c.

J. HORSBURGH.

P. S. Since I wrote the foregoing abstract, I have received a letter from my friend Mr. Dalrymple, intimating that a copy of the meteorological journal itself would be acceptable, which has induced me to transmit to him the original sheets, with a request to deliver them to you. I regret that I could not find leisure time to make out a fair copy, to have sent to you, in place of the original sheets in their rough state.

Bombay, June 1, 1804.

Second

V.

Second Communication on Artificial Tan. By CHARLES HATCHETT, *Esq. Abridged from the Philosophical Transactions for* 1805.

§ I.

THE artificial tan *, procured as described in the first communication (*see our Vol. XII. p.* 327), had been named tannin by Mr. Hatchett; but the objection having being made to this, that tannin was destroyed by the nitric acid, while the artificial tanning substance was actually formed by it, induced Mr. Hatchett to expunge the word *tannin* wherever it had been applied to the latter. It also induced the author to make the following experiments on the comparative effects produced by nitric acid on those substances which contain most tannin, and also some others in which a tanning substance has been produced, under circumstances in some respects different from those described.

Name of the artificial tanning substance altered.

§ II.

Although it is not absolutely asserted that the tanning substance is indestructible by nitric acid, yet the following experiments prove, that to produce this effect must at least be the work of much time and difficulty.

1. Twenty grains of the artificial tan were dissolved in half an ounce of strong nitric acid, of the degree of 1.40; the solution distilled till the whole of the acid came over, which acid was returned back on the residuum, and the distillation repeated three times in this manner. Care was taken not to overheat the residuum; and then, when examined, did not appear to have suffered any alteration in its properties.

Experiments to prove that the artificial tan is nearly indestructible by nitric acid.

* In several parts of the abridgement of Mr. Hatchett's papers, the artificial tanning substance has been called *the new tan* and *artificial tan*, and tanning matter *tan*, for the sake of brevity. It was thought necessary to mention this, as the name tan is usually appropriated to oak bark in a certain state; which, with singular impropriety, is that in which it contains least tanning matter, after having been used in the tanners' pits.—ABR.

2. Ten

Experiments continued.

2. Ten grains of the new tan, mixed with ten grains of white ſugar, diſſolved in half an ounce of nitric acid, was diſtilled to dryneſs. The reſiduum was not changed by the gelatinous or other re-agents.

3. This experiment was the ſame as the former, only that gum arabic was employed in place of ſugar. The reſult was the ſame.

4. The precipitate from a ſolution of iſinglaſs, with which the artificial tan had been mixed, was well waſhed with diſtilled water and then dried. In this ſtate it was digeſted in ſtrong nitric acid, by which a dark-brown ſolution was formed; which was evaporated to dryneſs, and the ſubſtance, diſſolved in boiling diſtilled water, was examined by nitrate of iron, acetite of lead, muriate of tin, and ſolution of iſinglaſs, with all of which it threw down copious precipitates, ſimilar in all reſpects to the artificial tan, which had not been ſubjected to the proceſs deſcribed.

5. Some of the precipitate of iſinglaſs by the new tan was diſſolved in muriatic acid, and evaporated to dryneſs: of this boiling diſtilled water diſſolved only a part; and the ſolution, of a dark beer colour, did not precipitate gelatine, though it acted on muriate of tin and ſulphate of iron; for with the former it gave an aſh-coloured precipitate, and with the latter a ſlight depoſit of a reddiſh-brown.

6. As boiling water diſſolved only a part of the iſinglaſs precipitate in the former experiment, the remainder was treated with nitric acid; after which, on being evaporated to dryneſs, it was found to be completely ſoluble in water, and precipitated gelatine as copiouſly as at firſt.

7. Twenty grains of the new tan was diſſolved in half an ounce of muriatic acid: The reſiduum, after evaporation to dryneſs, appeared in every reſpect unchanged.

The author here makes the obſervation, mentioned at the concluſion of the former paper, relative to the ſolutions of the new tan not becoming mouldy like thoſe of galls, ſumach, and catechu, and ſeeming to be completely imputreſcible.

And having thus aſcertained the unchangeable nature of this ſubſtance, he made the following comparative experiments on galls, ſumach, Pegu cutch, kaſcatti, common cutch, and oak bark.

8. Twenty

8. Twenty grains of powdered galls were dissolved in half an ounce of strong nitric acid: The residuum from this solution evaporated to dryness, and then dissolved in boiling water, did not produce the smallest effect on dissolved gelatine.

Experiments on galls, sumach, cutch, kascatti, Pegu cutch, and oak bark.

The experiments on to No. 13. did not produce any tannin.

9. The residuum of a strong infusion of galls, treated as No. 8.

10. Isinglass precipitated by infusion of galls, dissolved in strong nitric acid, and examined as No. 4.

11. Twenty grains of sumach dissolved in half an ounce of strong nitric acid, and treated as No. 8.

12. Twenty grains of Pegu cutch (which contains much mucilage) subjected to a similar process, by which much oxalic acid was obtained.

13. Twenty grains of catechu, called kascatti, treated similarly, had, together with the four foregoing experiments, all the same results as No. 8, not any of them shewing any tannin.

14. Twenty grains of common catechu, dissolved in strong nitric acid, evaporated to dryness, dissolved in water, and examined by isinglass, deposited a tenacious film insoluble in boiling water, evidently composed of gelatine and tannin.

15. Twenty grains of oak bark treated in the same way, deposited also an insoluble film on the sides and bottom of the vessel.

16. Infusions of galls, sumach, and oak wood, of equal strength, were mixed with nitric acid, in the proportion of half an ounce measure of each to one drachm of the acid, and did not then render isinglass solution turbid.

But infusions prepared from oak bark and the artificial tan, and managed in the same way, continued to precipitate the gelatine, until four drachms of the nitric acid had been added to each half ounce of the infusion.

These results shew that artificial tan is the most indestructible, but that the other tanning substances have considerable varieties in this respect. The tannin of oak bark resists nitric acid longer than that of galls, sumach, kascutti, or Pegu cutch. This last is replete with mucilage, and yields much oxalic acid, as before described: it seems also to be the most destructible of all the kinds of catechu: From these facts the author was induced to add the sugar and gum to the artificial

tan,

tan, to promote its destructibility; and expresses his belief that mucilage or gum renders the substances that contain it more destructible in the nitric acid, and in some cases also prevents or impedes the formation of the tanning substance; which difference he thinks to be caused by the mucilage being in a state of chemical combination in those bodies.

§ III.

Experiments on the artificial tan.

A and B. When sulphuric or muriatic acid was added to a solution of the new tan, it became turbid and deposited a brown precipitate, which was soluble in boiling water, and was then capable of precipitating gelatine; in which particulars it resembles the tannin of galls and other vegetable substances.

C. Carbonate of potash, added to a solution of the new tan, deepened the colour; the liquor became turbid, and deposited a brown magma.

D. Five grains of dried artificial tan were dissolved in half an ounce of strong ammonia: the whole was then evaporated to dryness; and being dissolved in water was found not to precipitate gelaten, unless a small portion of muriatic acid was previously added.

E. Another portion dissolved in ammonia was distilled: At first ammonia came over, and afterwards a yellow liquor, that had the odour of burned horn. The residuum was insoluble in water, to which it only gave a slight yellow tinge.

On distillation it has an odour of burned horn, and yields ammonia.

F. The object of this experiment is to shew the strange property of the new tan, of giving products analogous to animal matter (of which it yielded the odour in combustion on former trials), though prepared itself from vegetable substances. Some prepared from dry vegetable charcoal was distilled: First a little water came over, then a little nitric acid, then a very small portion of a yellowish liquor: The fire being then raised, the vessels suddenly became filled with a white cloud, and so great a torrent of gas was almost explosively produced as to overset the jar: This gas, by its smell, appeared to be ammonia, and was formed into the cloud by the nitric acid vapour in the vessels. The next jar of gas, which came slowly over, was carbonic acid, except a very small part which seemed nitrogen gas. A bulky coal remained, that on incineration gave $1\frac{1}{2}$ grains ashes, which consisted principally of lime.

G. Fifty

G. Fifty grains of this fubftance were diffolved in four ounces of water and precipitated by ifinglafs folution; eighty-one grains of which became thus combined with forty-fix of the new tan. The remaining portion was not precipitated, and was therefore feparated on a filter and evaporated to drynefs. It was a light brittle fubftance of a pale cinnamon colour, which, though compofed of inodorous fubftances, had however a ftrong fmell itfelf of oak bark; which is remarked as a fingular circumftance; and this fmell became ftronger when the fubftance was put into water, in which it inftantly diffolved.

The folution was very bitter; acted but flightly on diffolved ifinglafs; produced a brown precipitate with fulphate of iron, and with muriate of tin a black one; had no effect with nitrate of lime; but with acetite of lime gave a copious precipitate, of a pale brown colour. This fubftance appeared to be the tanning matter in the ftate of extract.

§ IV.

Several unfuccefsful attempts were made to form the tanning matter by oxi-muriatic acid. It therefore appeared, that though a variety of it could be produced by the action of fulphuric acid on refinous fubftances, yet nitric acid was the moft effective agent. Attempts to form tanning matter by oxi-muriatic acid unfuccefsful.

The author fufpecting that the new tan might be formed from bodies not abfolutely converted into coal, and not being able to get any touch-wood, which he firft thought of trying for this purpofe, made the following experiment with indigo, which he knew to contain much carbon. Artificial tan, it is fufpected, might be formed from fubftances not charred.

One hundred grains of indigo, with one ounce of nitric acid diluted with a double quantity of water, was (when the effervefcence had fubfided), placed in a fand-bath for feveral days till evaporated to drynefs. Experiments on indigo with this view.

The refiduum, of an orange colour, was in great part diffolved by three ounces of diftilled water poured on it, and gave a folution of a deep yellow, and intenfely bitter; which, with the fulphate of iron, depofited a flight pale-yellow precipitate, and with nitrate of lime, a fmall white precipitate, having the character of oxalate of lime: With muriate of tin a copious white precipitate, that changed to a yellowifh-brown;

brown; and with acetite of lead a beautiful deep lemon-coloured precipitate, which may probably prove useful as a pigment.

Ammonia rendered the colour much deeper, and with it deposited a large quantity of fine yellow spiculated crystals, which did not precipitate lime from its solutions. Their flavour was very bitter.

It produces tanning matter.

Lastly, when this solution was added to dissolved isinglass, it became turbid, and deposited a tough elastic insoluble film, and possessed the characters of gelaten combined with tanning matter.

Almost all vegetable bodies yield tanning matter, when subjected to repeated distillations with nitric acid.

By this experiment the possibility of producing tanning matter from bodies not converted into coal was fully ascertained; and the author has since discovered that though indigo yields this matter more readily than most other vegetable bodies, yet almost all produce it when subjected to repeated distillations with nitric acid.

Resin yields it by this treatment.

A. The common resin did not produce the tanning substance with nitric acid, but by the aid of sulphuric acid, as before related; yet upon this nitric acid being repeatedly abstracted from it, its solution in water formed a tough yellow insoluble precipitate with dissolved gelaten, similar to that by solution of indigo, and with other re-agents produced the following effects.

With sulphate of iron, after 12 hours, it produced a slight yellow precipitate. With nitrate of lime no effect. With muriate of tin, after 12 hours, a pale brown precipitate. And with acetite of lead a very abundant precipitate of a yellowish white colour.

On repeating this experiment, the author remarked that during each distillation nitrous gas was produced, while the acid which came over was weakened, which made the cause of the change in the properties of the resin evident. The following are the results of experiments tried with other resinous substances.

As do likewise stick lac,

B. Stick lac, treated as described, copiously precipitated gelaten.

—and balsam of Peru,

C. Balsam of Peru during the process afforded some benzoic acid, and gelaten was precipitated by the aqueous solution.

Benzoin

Benzoin, after the sublimation of some benzoic acid, yielded a residuum, which yielded with water a pale yellow solution, of a very bitter flavour. —and benzoin,

This solution with sulphate of iron produced a slight pale yellow precipitate. With nitrate of lime, no effect. With muriate of tin in solution, a small quantity of brownish white precipitate. With acetite of lead, a copious pale yellow precipitate. And with solution of isinglass, a dense pale yellow insoluble precipitate.

E. Balsam of Tolu afforded benzoic acid, and the solution of the residuum precipitated that of gelaten.

F. One hundred grains of dragon's blood in powder mixed with one ounce of nitric acid, evolved much gas; an ounce of water was then added; and the digestion in a sand bath being continued, after it produced chaficcation on the dry yellow mass that remained, a brilliant feather-like sublimate arose, which weighed rather more than six grains and had the aspect, odour, and properties of benzoic acid. —and dragon blood,

The residuum, of a brown colour, formed with water a gold coloured solution, which was not affected by nitrate of lime: But with sulphate of iron, and with muriate of tin it formed brownish yellow precipitates; and with acetite of lead one of a lemon colour.

Gold was precipitated by it in the metallic state, and the containing glass coloured purple; and with dissolved isinglass it produced a deep yellow insoluble deposit.

As dragon's blood simply exposed to heat did not produce any benzoic acid, the author is inclined to believe, that in the first experiment this acid was obtained as a product, and not as an educt.

G. Gum ammonia gave a brownish yellow, bitter astringent solution; which with sulphate of iron became of a darker colour, but produced no precipitate. —and gum ammonia,

With nitrate of lime, a slight precipitate. With muriate of tin and acetite of lead, copious yellow precipitates; and with gelaten a bright yellow insoluble deposit.

H. Assa fœtida yielded a solution which precipitated gelaten in a similar manner to that described. —and assa fœtida.

I. Solutions of elemi, tacamahac, olibanum, sandarach, copaiba, mastich, myrrh, gambauge, and caoutchouc, although Solutions of elemi, tacamahac, &c. did not affect gela

though they precipitated the metallic solutions, did not affect gelaten; but possibly might have done so, if the process had been more frequently repeated.

—not that of sarcocol,

K. Sarcocol also produced similar results.

—nor of gum Arabic,

L. Gum Arabic afforded oxalic acid but no tan.

—nor of tragacanth,

M. Tragacanth yielded much of saclatic acid, of oxalic, and of malic acid, but not the least tan.

—nor of manna.

N. Manna gave oxalic acid, part of which sublimed in the neck of the vessel.

Its residuum formed a brown solution, which produced precipitates of the following colours: With sulphate of iron, a pale yellow; with muriate of tin, a pale brown; with acetite of lead, a brownish white. From nitrate of lime, oxalate of lime was copiously precipitated by it; but with isinglass solution no effect was produced.

Liquorice solution precipitates gelaten.

O. Nitric solution of liquorice yielded precipitates with sulphate of iron and muriate of tin, after twelve hours, slight brown. With acetite of lead, a brownish red. With nitrate of lead, a brown. And with gelaten, one of a yellowish brown, insoluble, and similar to other precipitates from it by tan.

Guiacum solution gave a slight precipitate with gelaten, which was soluble in water.

P. On guiacum nitric acid acted with great vehemence and speedily dissolved it: The residuum was almost totally soluble in water; and this solution produced effects on the metallic salts similar to those recited; but with gelaten formed a slight precipitate, which was speedily dissolved by boiling water. The remainder of the solution evaporated gave a large quantity of crystalized oxalic acid; so that in this respect guiacum was similar to the gums, and unlike the resins.

§ V.

Experiments on several roasted vegetable substances, which do not affect gelaten.

As many vegetable substances when roasted yield a liquor by decoction, resembling solution of artificial tan, the author tried those similarly prepared, of dried peas, horse beans, barley, and wheat flour, none of which gave any precipitate with gelaten.

Coffee gives a precipitate with it, which is again soluble in water.

The decoction of coffee also gave no precipitate till after several hours, and then one soluble in boiling water; but this might be occasioned, the author thinks, from want of some particular nicety which may be required in roasting such bodies

ſo as to make them yield tan; which opinion was corroborated by experiments made by decoction of chicoree (probably endive) root, prepared in the ſame manner, which produced a precipitate with gelaten after ſome time, though not at firſt, which was apparently diſſolved in boiling water, but depoſited again in its original ſtate, on cooling. The author therefore is inclined to believe that the tanning ſubſtance is really developed in many vegetable matters by heat alone; but that a certain degree of heat, not eaſy to determine is abſolutely neceſſary for this effect.

A ſmall quantity of nitric acid added to any of the decoctions juſt mentioned, and evaporated to dryneſs, produced a reſiduum, having all the properties of the tan produced from coal. Nitric acid added to theſe decoctions gives tanning properties to their reſidue.

§ VI.

The production of a variety of the tanning ſubſtance before mentioned, by the action of ſulphuric acid on the reſins, amber, &c. ſuggeſted the following experiments on camphor; the reſults of which tend to increaſe the knowledge of its properties.

Experiments on Camphor with Sulphuric Acid.

The only facts hitherto related relative to the effects of ſulphuric acid on camphor, are that a brown or reddiſh brown ſolution is formed, from which water precipitates the camphor unchanged; but this only happens at a certain period of the operation; for if it be longer continued, the following effects will be produced.

A. One ounce of concentrated ſulphuric acid was added to one hundred grains of camphor, which diſſolved gradually, after firſt becoming yellow; in about an hour, the liquor having progreſſively changed to reddiſh brown, brown, and at laſt blackiſh brown, much ſulphureous acid gas was produced, and continued to encreaſe during four hours, when the whole appeared a thick black liquid, having no other odour but that of ſulphureous acid; after two days the production of the gas was much diminiſhed; the containing alembic was then put in a ſand bath, moderately hot, by which more ſulphureous gas was obtained; but this ſoon abated; at the end of two days more, ſix ounces of water was gradually added, by which the liquor Experiments with ſulphuric acid on camphor.

liquor changed to a reddifh-brown, a coagulum of the fame colour fubfided, the odour of fulphureous acid gas was immediately annulled, and was fucceeded by one which much refembled a mixture of oils of lavender and peppermint.

An odour yielded by it like oils of lavender and peppermint.

The whole was then diftilled gradually; when the water came over impregnated with the odour laft mentioned, accompanied by a yellowifh oil, which floated on the top, and was computed to amount to three grains.

B. When the whole of the water had come over, there was again a flight production of the fulphureous acid gas; two ounces of water were then added, and the diftillation continued (without the recurrence of the former odour) till a dry blackifh brown mafs remained; this mafs was well wafhed with warm diftilled water, by which nothing was extracted; but two ounces of alcohol digefted on it for 24 hours formed a very dark brown tincture.

The refiduum was digefted with two ounces more alcohol, and the procefs repeated till the alcohol ceafed to act.

The refiduum had now the appearance of a compact fort of coal in fmall fragments, which were well dried, and after being expofed to a low heat in a clofe veffel, weighed fifty-three grains.

C. From different portions of the alcohol folution, added together and diftilled in a water bath, a blackifh brown fubftance was obtained, which had the appearance of a refin or gum with a flight odour of caromel, and weighed 49 grains.

The products obtained from the 100 grains of camphor treated with fulphuric acid, were,—

Products from camphor and fulphuric acid.

	Grains.
A. An effential oil, having fomewhat of an odour of a mixture of lavender and peppermint, about	3
B. A compact and very hard fort of coal, in fmall fragments, - - - - -	53
C. A blackifh-brown fubftance, of a refinous appearance, - - - - -	49
	105

The increafe of weight of five grains is attributed partly to water retained by the laft fubftance, and partly to oxigen united to the carbon.

The fubftance C had the following properties:

1. It

1. It was bitter and aftringent, had the odour of caromel, and formed with water a dark-brown folution.

2. This folution produced very dark-brown precipitates with fulphate of iron, acetite of lead, muriate of tin, and nitrate of lime.

3. Gold was precipitated by it in the metallic ftate from its folution.

4. By folution of ifinglafs the whole was precipitated; fo that after four hours a colourlefs water only remained.

Subftance C (from camphor) precipitates gelaten.

This precipitate was nearly black, and was infoluble in boiling water: from whence, and its effects on fkin, it was evidently a variety of tanning matter much refembling that obtained from refinous bodies by fulphuric acid.

But this fort of tan had lefs effect on fkin than that procured from carbonaceous fubftances by nitric acid, and its precipitate from gelaten was more flocculent and lefs tenacious.

However, when a fmall quantity of nitric acid was added to the folution of the fubftance obtained from camphor, and when the refiduum, after evaporation to drynefs, was diffolved in water, a reddifh-brown liquor was formed, which acted in every refpect fimilar to the tanning fubftance obtained from the varieties of coal by the nitric acid.

§ VII.

The three varieties of artificial tan.

From the experiments related, it appears that three varieties of the tanning fubftance may be formed.

1ft. That produced by nitric acid with any carbonaceous fubftance, whether vegetable, animal, or mineral.

2d. That formed by diftilling nitric acid from common refin, indigo, dragon's blood, and various other fubftances.

3d. That which common refin, elemi, affafœtida, camphor, &c. yield to alcohol, after they have been previoufly digefted with fulphuric acid.

Remarks on them.

On thefe products the author makes the following remarks: The firft variety is the moft eafily formed. From fome experiments made purpofely it appears, that, after making allowance for a fmall quantity of moifture and of nitric acid remaining, 100 grains of vegetable charcoal yield 116 of the dry tanning fubftance.

100 grains charcoal yield 116 tanning matter.

Carbon the base of the tanning matter.

From the manner in which it is produced, carbon is evidently the base and predominating essential ingredient in this substance.

It also contains oxigen, hidrogen, and nitrogen.

From § III. experiment F, it also appears that the other component parts are oxigen, hidrogen, and nitrogen; for when the artificial tan was distilled, ammonia and carbonic acid were obtained, exclusive of a small portion of a yellow liquor that appeared to be of an oily nature, from being insoluble in water and alcohol.

It has an odour like animal matter when burned, and one of oak bark when precipitated as in G, § III.

Many of the properties of the tanning substance prepared from coal by nitric acid are very remarkable, particularly those noticed in § III. experiment F; of its having the odour of animal substances when burned, though prepared from vegetable matter; and in experiment G, of the precipitate having the odour of oak bark, though the component materials were inodorous.

It resembles vegetable tannin in most properties.

But its most extraordinary properties are those in which it so nearly approaches the vegetable *tannin*, which it perfectly resembles in its solubility in water and in alcohol, in its action on gelaten and on skin, in its effects on the metallic solutions, on the alkalis, and on the earths.

Difference between it and tannin.

The sulphuric and muriatic acids also affect its solutions, as they do those of tannin; and the only marked difference between artificial tan and tannin is, that the former is produced by nitric acid, while the varieties of the latter are more or less destroyed by it; but here it must be remembered, that even the varieties of tannin do not accord in the degree of destructibility.

Second variety of artificial tan.

The second species of the tanning substance is obtained from a variety of vegetable bodies before recited, by digesting and distilling them with nitric acid. It is therefore not so readily prepared, and the quantity of it produced is less in proportion to the substance from which it is prepared.

Theory of its formation.

As resin and some other bodies do not afford it until they have been repeatedly treated with nitric acid, and as, during each operation, nitrous gas is produced, while the strength of the acid which comes over is diminished, the author thinks it almost certain that the tanning substance is formed in consequence of part of the oxigen of the nitric acid becoming combined with the hidrogen of the original body, so as to form

water:

water; and the carbon being thus in some measure denuded, is rendered capable of being acted on gradually by the nitric acid, in a manner nearly similar to what takes place when it has been previously converted into coal.

The precipitates of this tanning substance from gelaten are always pale or deep yellow, while those formed by the first species are constantly brown; which induces the author to believe that the different colours of the precipitates depend on the state of the carbon of the tannin.

The quantity of artificial tan obtained from resin and other bodies, was always less than that from coal, or even from the same bodies previously converted to coal in the humid way by sulphuric acid. The cause of this seems to be, that a number of other products are simultaneously formed with the tanning substance, all of which require more or less carbon as an ingredient; so that, according to the affinities which prevail, some bodies afford but little, and others none of it.

Quantity of artificial tan less from resins than from coal, accounted for.

The greatest proportion of this substance was yielded by indigo, common resin, and stick lac.

Its proportions from different substances.

The quantity obtained from assafœtida and gum ammoniac was less.

Benzoin, balsam of Tolu, balsam of Peru, and dragon's blood, were inferior to the former in this respect; so that the production of benzoic acid seemed to counteract the formation of the tanning substance. But oxalic acid, when formed in any considerable quantity, seemed absolutely to prevent the formation of this substance: for gum arabic, tragacanth, manna, and guiacum, which produced oxalic acid in abundance, yielded no tanning matter.

Benzoic acid when formed counteracts the formation of the tan; and oxalic acid, when produced, prevents it entirely.

Common liquorice seems to be an exception; but the author supposes that the small quantity of tan produced by it, was formed by the action of the nitric acid on a portion of uncombined carbon, which being in a state approaching to coal, is probably the cause of the blackness of the common liquorice.

The third variety of the tanning substance appears to be uniformly produced during a certain period of the process; but by a long continuance of the digestion there is reason to think it is destroyed.

Third variety of artificial tan.

Substances, such as gums, which yield much oxalic acid, do not apparently afford any of this tanning matter.

The energy of its action on gelaten and skin is certainly inferior to that of the first variety, into which however it may easily be converted by nitric acid.

From the mode of its formation, there does not appear to be any evidence of its containing nitrogen like the first and second varieties, and perhaps the absence of nitrogen may be the cause of its less powerful action.

Experiments of Messrs. Biggin, Proust, and Davy on tannin, noticed.

In the course of the communications on this subject, Mr. Hatchett notices the experiments on tannin by Mr. Biggin, the great contributions of M. Proust to the elucidation of its nature and properties, and the very great extension of, and valuable additions to the same, from the ingenious labours of Mr. Davy, particularly his discovery of the singular fact that terra japonica, or catechu, consists principally of tannin.

Medicine, arts, &c. may derive great benefit from farther investigations of gums, resins, &c.

The author also greatly recommends the farther investigation of the nature of the gums, resins, balsams, and gum-resins, by every possible method; and is of opinion, that medicine, arts, and manufactures may derive many advantages from it, and the mysterious processes of vegetation probably receive considerable elucidation.

VI.

On carbonised Turf. From a Report made to the Prefect of Police (at Paris) on the Methods employed for reducing it to this State. By MM. CALLIAS *and Co**.

The use of turf very ancient. It produces no deleterious effects.

THE use of turf for domestic fuel is of a very ancient date: Some of the most eminent men of science have pronounced that it does not produce any deleterious effects. Without citing the examples of England (Ireland), Scotland, and Holland, where great quantities of it are consumed, we will confine ourselves to the use made of it in France, in the *(ci-devant)* provinces of Flanders, Artois, and Picardy.

Its use is now tolerated in Paris to relieve the scarcity of wood: the lime-burners, plaster-bakers, brick-makers, and washers, make great use of it both in the city and its vicinity; and it has never been perceived that those who lived within

* Sonini's Journal, Tom. II. p. 324.

the

the influence of its smoke, have experienced any bad effects from it. The commissioners (employed to make this report) observe, that the great volume of smoke which is disengaged on the commencement of its combustion, is only caused by a great portion of water contained by the turf, which is expanded into steam by the heat; soon afterwards this smoke is combined with an acid analogous to that of vegetable substances, which, far from making the air deleterious, tends on the contrary to neutralize the vapours of infection which it may contain. It is true that sometimes, for an instant, the turf in combustion exhaled an odour of empyreumatic oil, in the form of gaseous vapours, but this odour is by no means injurious to the animal organisation, but, on the contrary, is beneficial in nervous affections.

Its smoke is caused by contained water; with an acid vapour, which removes infection.

But if this odour is disagreeable when the turf is burned in towns, villages, and private houses, this complaint cannot take place when it is burned in the open air at a distance from all habitations, which will be effected by its previous carbonisation, as managed by MM. Callias and Co. therefore the company merit the public protection.

Charing prevents all the unpleasant effects of its odour.

In 1785 the French Government took a great interest in what related to the carbonisation of turf, and granted 80,000 francs to a company to erect a furnace for this purpose on the ground of the Capuchins. The method of this company was that of extinguishing, but their plan did not succeed, and the works were abandoned.

Company at Paris for charing turf, assisted by the government, did not succeed.

A new company tried, some time after, the same enterprize, at its own expence: the method of operating in closed vessels was proposed: the experiments made were on a great scale, and were attended with a success that was certified by the commissioners of government: a memoir printed in 1790, by M. Morelot, contained these facts, with a statement of the superiority of turf-charcoal over that of wood. But the disastrous events of the revolution put an unhappy end to this enterprize which promised so well.

Another company—promised well—but the revolution caused its failure.

At present MM. Callias and Co. offer to the public an additional species of fuel to that hitherto in use, a charcoal of a new process, the materials of which are spread with profusion over the territory of France, and the consumption of which, being substituted for that of wood, will at the same time be an object of economy to individuals, and of incalculable advantage

Callias and Co. charr turf by a new process.

The use of turf charcoal. It will be cheap,

and prevent the destruction of the forests.

vantage to the management of the forests. Timber for the construction of houses and furniture, and timber for ship-building, daily increase in price, because they become more scarce. Some of the forests have become reduced, as the fresh growths in them do not keep pace with the destructive instrument that overturns them; some of them are entirely destroyed, and the ground converted into ornamental gardens; and thus each year, each month, each day, conducts us insensibly to a most alarming dearth of timber. Already the price of fire-wood is tripled, and Paris is on the eve of being deprived of a combustible which, as yet, has not been replaced to advantage.

The carbonization used by Callias is very perfect.

The commissioners compliment MM. Callias and Co. whose method of carbonisation is peculiar to themselves, and calculated conformably to the laws of combustion in its two first stages; that is to say, before the arrival of its third degree, or that of absolute combustion. MM. Callias and Co. by their method, direct the carbonisation at their pleasure and in an invariable manner; they are always sure of obtaining a perfect charcoal, without smoking-pieces, and without any risk of forming it into a pyrophorus, which sometimes happens in the carbonisation performed in closed vessels. Their manner of proceeding is also very economical; and what proves that they work with intelligence is, that they daily improve, and already are able to save ten hours out of 48 in each carbonisation.

Experiments made with Charcoal of Turf.

Turf-charcoal yields more heat than wood-charcoal.

1*st*. The charcoal of turf kindles a little slower than that of wood, but when it is once in compleat ignition, it throws out much more heat; its flame is also more elevated, and it yields no odour, except a very slight one of sulphur, which ceases when it is fully lighted.

Causes water to boil four times as speedily.

2. Charcoal of turf, in equal quantity with charcoal of wood, caused a given quantity of water to boil four times; while that of wood caused it only to boil once. The first is then superior to the second in a quadruple proportion.

3. To prove that turf charcoal emits more heat than wood charcoal, the following experiment was made.

It fused 11 oz. of gold in eight minutes; wood-charcoal did the same in sixteen.

With turf charcoal, in a goldsmith's furnace, eleven ounces of gold were fused in eight minutes, which with wood charcoal was not performed in less than sixteen minutes. The gold lost nothing of its malleability in the fusion with the turf; but,

on

on the contrary, it was neceffary to add fome reductive flux, to that fufed by the wood charcoal, in order to reftore the malleability which it had loft.

minutes. The malleability of the gold was preferved.

4. Iron made red-hot by charcoal of turf in a forge, became more malleable; which proved that it gave none of its carbon to the metals with which it came in contact.

Iron heated by it becomes more malleable. It lafts longer than wood-charcoal.

5. Finally, Turf charcoal lafts longer in a ftate of ignition than charcoal of wood, and its heat is conftantly equal.

Conclufion.

1. The odour of turf in combuftion is noways deleterious. This truth has been confirmed by the moft diftinguifhed chemifts; and is befides proved by the conftant ufe made of this fuel in the *ci-devant* provinces of Flanders, Artois, and Picardy.

Its odour not at all unwholefome.

2. It is defirable that the carbonifation of turf may be encouraged, on account of the great advantages which may refult from the ufe of this new fpecies of charcoal, both for private confumption and for large works.

Its ufe ought to be encouraged,

But the greateft matter in its favour is, that its ufe tends to diminifh the felling of the forefts, whofe extenfion ought to be promoted by every means poffible, and which nothing tends fo much to deftroy as the ufe of wood charcoal.

and will preferve the woods.

VII.

Account of the Cataracts and Canal of Troellhætta, in Sweden, (from a Work relative to them by Colonel SKIOELDEBRAND. *Publifhed in one Volume Quarto, at Stockholm.)*

THE cataracts of Troellhætta produce one of the fineft effects which nature affords in Europe. The river of Gothie is the only outlet of the vaft lake of Wener, navigable through its whole extent. This river, which falls in the North fea near Gothemberg, as foon as it departs from the lake, which is much more elevated than the fea, rolls its waters with impetuofity, and dafhes them againft fteep rocks, whofe refiftance forms a fucceffion of cataracts, which without being individually very high, form altogether a moft ftriking object. The imagination is the more affected by this fight, as the furrounding fcenes are of a dark and melancholy character, confifting of grey rocks,

The cataracts are formed by the river Gothie foon after its departure from lake Wener.

They form a very ftriking object.

crowned

crowned with ancient firs, and of frightful precipices, formed by the bursting of the locks and banks, which the fury of the water has overturned. These last were constructed in the bed of the cataracts, in order to render the river navigable through its whole length; but this daring work of man could not resist the reiterated efforts of nature, and therefore it was necessary to have recourse to another plan.

Ruins of locks which had been formerly constructed in the bed of the cataracts.

The canal newly constructed passes by the side of the cataracts, and its bed is partly formed in the natural rock, and partly in a marshy soil. It was began in 1794, and finished at the end of six years, in 1800. Its breadth is 22 feet, and its depth six feet and an half. Its locks are eight in number, and its cost amounted to the sum of 59833 pounds sterling, which was collected by subscription. By means of this canal there is a continued navigation, without any interruption, from the province of Wermeland to Gothemberg. In 1802 the number of vessels which had passed this canal amounted to 1380, which is at the rate of 1190 each year.

The canal passes by the side of the cataracts. Is partly cut in the rock. Its breadth, depth, number of locks, time of excavation, and cost. The extent of its navigation, and number of vessels which have passed.

VIII.

Letter from H. B. K. *on the Production of Nitrous Acid, and other Facts**.

To Mr. NICHOLSON,

SIR,

As Mr. Accum has not answered my paper, he therefore knows of no experiments which shew the formation of the nitrous acid; but anxiously impressed with the subject, I have been performing some experiments, which I think will throw great light upon the cause of the nitrous acid appearing in electrical experiments.

Experiments announced. Carbonate of potash in water was galvanized, and emitted carbonic gas.

I passed the galvanic fluid through a watery solution of the carbonate of potash, made by distilled water, confined in a glass tube where no atmospherical air could have access to it, and I found a great production of air come from the solution, which upon examination was pure carbonated air; and

The potash became capable of deflagration like nitre; and solution of silver shewed the presence of muriatic acid.

* See our Journal, X. 105, 214, and XI. 105.

then

then examining the folution by dipping a piece of paper into it; upon its being dipped the paper fhewed evident figns of nitre upon it, and when burned it detonated the fame as nitre would have done; and alfo with the folution of filver the alkaline folution gave fome faint indications of the marine acid being prefent in it. That the folution fhould give indications of poffeffing both the nitrous and marine acids is not fo furprizing; as we have the fame products in firing oxigen and hidrogen gafes, according to the foreign experiments, principally the nitrous acid, but with it a fmall quantity of the marine acid.

Remark. Thefe two acids are found on the common detonation of oxigen and hidrogen.

I then filled the tube (after wafhing it clean) with pure diftilled water, and fent through it the galvanic fluid; and I obferved a generation of airs, which, upon examination, appeared to be the airs ufually formed in thefe experiments, as they exploded.

Pure water galvanized gave thefe gafes.

After this I filled the tube with a folution of pure potafh in diftilled water, and no air came from it upon galvanifing it; if any, it was carbonated air: But upon examining the folution, it gave clearly the fame indications of poffeffing the nitrous and marine acids, as the carbonated folution of potafh did in the firft experiment.

Pure potafh and water gave no gas by galvanizing, but indicated the two acids of the firft experiment.

Now exempt from all hypothefes, let us examine thefe interefting facts: The carbonated potafh had its carbonic acid air expelled, clearly from the acid or acids; as we know that it could not part with its carbonic acid air, but from the action of a ftronger acid. Alfo another more effential fact it proves to us, that the generation of the peculiar airs, what are called oxigen and hidrogen gafes, are owing to the acids; for when the potafh was in the water as to arreft or attract them, there were neither of thefe airs produced; and upon examining the diftilled water (in the experiment in which they were produced) after their production, there was no acid in the water, but it was pure diftilled water. Therefore, beyond a doubt, the nitrous acid is effentially concerned in the production of thefe peculiar oxigen and hidrogen gafes: indeed Mr. Cruickfhanks fays, that upon fufing thefe airs, he found in the refiduum the nitrous acid.

Remarks. The carbonic acid was expelled from potafh by a ftronger acid produced. Whence it is inferred that the oxigen and hidrogen arofe from thefe acids.

Thefe experiments were performed by two fhort gold wires attached to each end of the galvanic pile. But upon placing a pretty long iron wire to the filver end of the pile inftead of

The conducting wires were gold.

the gold one, a little hydrogen gas was produced; even when the potash was mixed with the distilled water, though there was none when it was a gold wire.

I hope Mr. Nicholson you will not refuse the insertion of these interesting facts in your Journal, for I have made the relation of them as brief as possible that they might not occupy too much room *.

H. B. K.

London, August 15.

IX.

Report of M. DEBUC'S *Memoir on Acetic Acid, made by M. M.* PLANCHE *and* BOULLAY, *by Order of the Society of Pharmacy at Paris* †.

M. Debuc repeated M. Badollier's process to obtain acetic acid from acetate of lead by sulphate of copper.

. Debuc saw in the *Annales de Chimie*, No. 109, a method of M. Badollier, apothecary at Chartres, for obtaining acetic acid very readily from a mixture of equal parts of sulphate of copper and acetate of lead by a moderate heat.

Relying on this process M. Debuc made an exact mixture of two pounds of sulphate of copper and an equal quantity of acetate of lead, which he exposed in a distilling apparatus on a sand bath to a moderate fire, which he increased by degrees during the operation, which lasted for six hours: the product obtained was 26 ounces. It was given to a manufacturer, without examination, and being used in his business produced an effect entirely contrary to the acid extracted from crystals of copper.

The product used in manufacture produces an effect contrary to the common acetic acid.

The process again repeated, and its results carefully examined.

This circumstance determined M. Debuc to repeat the process as before, and examine its results carefully: In which he observed the mixture of the two salts to become pastey, which is easily explained by the difference of the concentration of the acid in the sulphate of copper, from that in the sulphate of lead. The products of this experiment were,

* I am extremely sorry that this communication was by mistake placed among papers already printed; which alone has caused the delay in its appearance.—W. N.

† Annales de Chimie, Tom. LIV. p. 145.

1. Four

1. Four ounces of water slightly acidulated.

2. Four ounces of a liquor more acid than the first, and which M. Debuc compares to good vinegar of *Saumur*.

3. Eighteen ounces of a very limpid liquor, with a lively and penetrating odour of acetic acid mixed with sulphurous acid.

Products of this process.

The residue, weighing 38 ounces, appeared to M. Debuc in different layers more or less red, according to their distance from the bottom of the retort; and he found the upper part covered with a whiteish powder, slightly inclined to a citron colour, in which he recognised the presence of sulphur.

The residue is in layers of different colours.

Barytes, the muriate of lime, and the acetate of lead formed immediately considerable precipitates with the third product.

Precipitates formed with the third product by different salts.

M. Debuc observes that the decomposition of the acetate of lead by the sulphate of copper may be easily explained; but that here there is a production of sulphurous acid, and a decomposition of the sulphuric acid from the absorption of its oxigen by the vinegar; which is a singular phenomenon, that has no agreement with the affinities of the acidifying principle for the *acidifiable* and *salifiable* bases; he leaves the explanation of this matter to more experienced chemists, and only notices that the transportation of the oxigen of the sulphuric acid to another base, suggests the idea, *that acetic acid is superoxigenated vinegar*.

Sulphurous acid being produced, induces M. Debuc to suppose that acetic acid is super-oxigenated vinegar.

M. Debuc succeeded in freeing his third product from the sulphurous and sulphuric acids, by letting it remain for about 24 hours, on twelve grains of salt of tartar, and about two ounces of black oxide of manganese pounded fine, and after that distilling it slowly; by this rectification he obtained a pound of pure acetic acid of a lively and agreeable odour, and of about 10 degrees specific gravity; which is one degree less than that of radical vinegar well rectified, obtained from acetate of copper.

M. Debuc's method of freeing the third product from sulphurous and sulphuric acids.

Acetic acid produced, one degree weaker than the common kind.

The author concludes from this,

1. That the product of two pounds of acetate of lead, treated with an equal quantity of sulphate of copper, is twenty-six ounces; of which four ounces is acidulated water, an equal portion strong vinegar, and eighteen ounces acetic acid altered by the sulphurous and sulphuric acids.

2. That the eighteen ounces, forming the third product, rectified as recited, does not differ from that drawn from crystals of acetate of copper, but by its less density.

M. Debuc concludes that the acetic acid produced only dif-

3. That

fers in strength from the common kind, and may be substituted for oximuriatic acid, in some cases to advantage.

3. That in many cases this acid may be substituted for oxi-muriatic acid, as an object of salubrity without possessing its inconveniencies.

The reporters repeat M. Badollier's process, with M. Debuc's modifications.

The reporters repeated the process of M. Badollier with the modifications advised by M. Debuc as follows.

They introduced a mixture of two pounds of sulphate of copper, and the same quantity of acetate of lead into a glass retort, placed it on a sand bath, and adjusted to it a tubulated receiver, which communicated with two bottles of a Wolf's apparatus; the first of which contained distilled water, and the second many pounds of lime water; from this last a tube was passed underneath a jar in an hydro-pneumatic apparatus: the retort was heated gradually to the end of the operation, which lasted more than 10 hours; and the following products were drawn from the receiver.

Products of their process from the receiver.

1. Eight ounces of a liquor similar to distilled vinegar, but with a less agreeable odour.

2. Ten ounces of a liquor with an unpleasing odour of acetic acid, more penetrating than the first, and not containing any trace of sulphurous or sulphuric acids.

3. Finally seven ounces of a liquor of great limpidity, with a very pungent odour of sulphurous acetic acid, and which did not precipitate muriate of barytes.

A considerable disengagement of an elastic fluid was observed, which became perceptible as soon as the retort began to run, and which lasted during the whole operation.

Carbonic acid gas evolved.

This gaseous fluid was absorbed almost totally by the lime water, forming with it a very abundant white precipitate, which, gathered on a filter, and dried, proved to be carbonate of lime: It weighed two hundred and fifty grains, which made the carbonic acid equal, according to the known proportions of this substance, to eighty-five grains; atmospheric acid alone passed under the jar mixed with some carbonic acid gas: no trace was perceived of hydrogen gas.

Many layers of different colours were found in the retort.

The first was of a beautiful green, surrounded with a circle of yellowish white towards the sides.

The second, much more thick, was of a red colour, greatly like copper in very small particles.

The third was a mixture of sulphate of lead and of copper apparently in the metallic state.

The

The last larger, which occupied the bottom, of a black colour, and shining, was a mixture of sulphate of lead and of charcoal.

The lowest layer of the residue is a mixture of sulphate of lead and charcoal.

The same experiment with the same quantities of the salts, was repeated a second time, with the precaution of reducing the sulphate of copper by disiccation to $\frac{11}{16}$ of its weight. The product from this was preferable to the other.

The same process again repeated; drying the sulphate of copper, the produce is better.

The second and third products were mixed and rectified on carbonate of potash and oxide of manganese, with the precautions indicated by M. Debuc: This rectification produced an acetic acid of nearly the same specific gravity as that afforded by simple distillation from crystals of copper, but of a less strong odour, less agreeable, and besides mingled with sulphurous acid.

Second and third products rectified. A weaker and less agreeable acetic acid produced, mingled with sulphurous acid.

The reporters think that M. Debuc is deceived in his theory, *"that acetic acid is vinegar super-oxigenated by the oxigen of the sulphuric acid passing to the vegetable acid,"* for he has not considered,

1. That the acetic acid is almost all obtained, before the sulphurous acid becomes perceptible.

Reasons why M. Debuc's theory is erroneous.

2. That the metallic oxides, which are the basis of the salts employed, have less attraction than sulphur to oxigen.

3. That the disengagement of the carbonic acid is much more likely to explain the matter.

The considerable production of carbonic acid, and the presence of charcoal in the residue, surprised the reporters the more; as *MM. Boddolier and Darac* (the first in his notice of the preparation of acetic acid; the other in a memoir, in other respects very interesting, on the difference of acetous and acetic acids,) positively assert that in the operation related, *there was no other gaseous production but that of part of the air contained in the vessels, especially no carbonic acid, and not an atom of charcoal in the residue.*

The production of carbonic acid and charcoal from the process, is contrary to the assertions of MM. Bodollier and Darac.

The result found by the reporters so different from that of *M. Darac,* in an experiment on which he supports his theory of the identity of the acetous and acetic acids, was so favourable to the theory of *M. Chaptal,* that they would have been induced to decide in favour of the opinion of the latter, if the following comparative experiments had not confirmed them in a contrary notion, and appeared to them one of those, of which M. Darac might most avail himself.

To

An experiment made, favourable to M. Darac's opinion.

To four ounces of pure concentrated radical vinegar (extracted from cryſtals of copper by heat alone) were added by degrees four ounces of ſemi-vitreous oxide of lead (litharge) in powder; which compleatly diſſolved in it by heat, there even remained an exceſs of acid, perceptible in the ſtrong odour of the ſolution. Being laid by to cool, it produced a very irregular cryſtalline maſs.

Four ounces of this maſs of acetic lead, mixed with an equal quantity of ſulphate of copper dried, were treated in a convenient apparatus. The acetic acid produced had an odour more penetrating and agreeable: but all the other phenomena were the ſame as with the acetate of lead; that is to ſay, there was an equal developement of carbonic and ſulphurous acids, and charcoal was found in the reſidue.

Which determined the reporters to conclude,

The reporters conclude that this acetic acid is always mixed with ſulphurous acid; of which it cannot be freed entirely by M. Debuc's proceſs; and never has ſo pleaſing an odour as the common kind.

1. That acetic acid formed by the diſtillation of a mixture of ſulphate of copper and acetate of lead, is always mixed with ſulphurous acid, which does not become perceptible till towards the end of the diſtillation.

2. That it cannot be compleatly deprived of this ſulphurous acid by the rectification propoſed by M. Debuc.

3. That the acid itſelf, totally deprived of the ſulphurous acid, is never of ſo lively and agreeable an odour, as that drawn from the cryſtals of the acetate of copper.

4. That it is preferable to dry the ſulphate of copper before it is uſed.

5. That MM. Boddolier and Darac, were miſtaken in ſuppoſing, that no carbonic acid was obtained in this operation.

The difference of acetous and acetic acids probably does not depend on the ſtate of their acidification.

6. Finally that the production of carbonic acid does not any more prove the decarboniſation of the acetous acid in becoming the acetic, than the ſulphurous acid proves the ſuperoxigenation of the vinegar; but on the contrary that it is allowable to conclude, that the difference of theſe two ſubſtances is not cauſed by their ſtate of acidification.

Account

X.

*Account of the Imperial Botanic Garden of Schœnbrunn, in the Vicinity of Vienna.**

IN 1753 the emperor, Francis the first, caufed a portion of ground behind the garden of the caftle of Schœnbrunn to be prepared for the cultivation of exotics, and of plants remarkable for their rarity or beauty. By the advice of the celebrated Van Swieten, the famous florift Adrien Steckhoven was invited to Schœnbrunn from Leyden, who caufed many green-houfes to be conftructed there, with a very large and beautiful hot-houfe, and various other buildings. At the fame time Richard Vander Schot, of Delft, was named firft gardener, and employed to convey to Vienna a great number of rare and exotic plants, brought up in different parts of Holland, and thus at the end of one year the garden was already rich in valuable plants.

The garden eftablifhed in 1753, by Francis I.

Put under the care of Adrien Steckhoven, —Vander Schot the firft gardener —brings to it many exotics from Holland.

M. Jacquin, who was then at Vienna, went to vifit the garden of Schœnbrunn, to clafs thofe plants which had not yet received a fpecific denomination; on which occafion he became known to the emperor, who propofed to him to travel at his expence on the continent of South America, and in the American iflands, to enrich the garden with plants from the moft diftant countries. Accompanied by the gardener Vander Schot, he departed from Vienna in 1754; and in paffing through Italy was joined by Jean Buonamici and Ferdinand Barculli, who were entrufted with the zoological part of the expedition, by which it was propofed to improve the royal menagerie, and the cabinet of natural hiftory at the fame time. After having vifited the iflands of Martinico, of Grenada, St. Vincent, St. Euftatia, St. Chriftopher, St. Martin, St. Bartholomew, Aruba, Cuba, Caracca, and Jamaica, he returned to Vienna in 1759. From Auguft 1757 to the middle of 1759, M. Jacquin could do little for the advancement of Science, having been ill of a lientery for four months, of which he was at laft cured at Jamaica. The war which then commenced between England and France, alfo deranged his travels.

M. Jacquin fent to America to collect plants.

Account of his proceedings in the Weft India iflands.

* Magafin Encyclopedique, T. 6, p. 552.

vels! The veſſel in which he made his voyage was taken, and he was thus obliged to paſs a conſiderable time, againſt his will, at Montſerrat and the deſert iſland of Gonave.

The firſt cargo of plants ſhipped,—the ſecond cargo brought by Vander Schot.

In the month of Auguſt 1757 the firſt cargo of plants to the garden of Schœbrunn was ſhipped from Martinico, which arrived at Marſeilles. In the month of February, 1757, Vander Schot returned alſo from Martinico, and brought with him from the ſame iſland a great quantity of trees and ſhrubs. All this cargo arrived ſafe, except ſome ſpecimens of *Aleliconia*, which were attacked on the voyage by mice. The trees were of the height of a man, and of the thickneſs of an arm, and ſometimes more. The moſt of them had born fruit in their native ſoil; their tops had been cut off, and only ſome of the principal branches were permitted to remain about two feet in length; the ſhrubs remained in their natural ſtate.

Method of preparing the trees for carriage,—and of packing them.

To remove thoſe trees from their native earth, a circular trench was dug round each, at a convenient diſtance, in ſuch manner that there might remain attached to their roots as great a maſs of the earth in which they grew as was poſſible. This maſs, which formed a ſort of ball, was entirely wrapped up in leaves of the *Muſa*, ſecured with cords made of the bark of the *hibiſcus tiliaceus*, in ſuch a manner that the earth could not fall out.

Weight of a tree when packed 100lb.

A ſingle tree packed in this manner, weighed commonly an hundred and odd pounds. The balls of earth were moiſtened a little, with the neceſſary care, and ſuſpended in the air, where the vegetation ſoon became apparent.

Method of tranſporting the packages.

To prevent the earth from being detached from the roots on the way, all the packages were tranſported in barks to the port of St. Pierre, in Martinico; from hence they were ſhipped to Marſeilles, and from thence brought by ſea alſo to Leghorn, and from this port were carried by mules to Schœnbrunn. This was without exception the richeſt cargo of living plants which had ever been brought from the hot countries to Europe.

The third cargo of plants ſhipped,—the fourth, and the fifth.

In the month of Auguſt, 1756, Buonamici ſet off with the third cargo from St. Euſtatia to Leghorn. The fourth cargo departed towards the end of the ſame year. The fifth was ſhipped from Curacao for Amſterdam, and was accompanied by J. A. Veſuntin, who died in Germany of the dyſentery. This cargo was extremely rich in corals and other productions of the ſea, which ſtill form ſome of the moſt precious ornaments of

of the Imperial Cabinet. In the fame year, M. Jacquin fent off the fixth cargo, from the fame ifland to Amfterdam. And finally, in January 1759, MM. Jacquin and Barculli departed with the feventh cargo from the Havannah, for Ferrol in Spain, and arrived at Vienna in the month of July. This laft cargo was particularly rich in animals of every fpecies.

The fixth cargo fhipped, —the feventh brought by MM. Jacquin and Barculli, 1759.

Thus in the fpace of a few years the number of plants in the garden of Schœnbrunn was confiderably encreafed; for, befides thofe which had arrived from America, means were found to make many important acquifitions in different other countries. In 1765, after the death of Francis the firft, Maria Therefa ordered the garden to be conducted on the fame footing that it was before. In 1780, a little while before the death of this princefs, it fuffered a fmall but irreparable lofs; the gardener, Van der Schot, then very aged, had been confined to his chamber for many weeks by an attack of the gout. Thofe to whom the management of the plants was entrufted in that period acquitted themfelves with great negligence; in one of the coldeft nights of that winter, the perfon who fhould have taken care of the great hot-houfe forgot to keep up the fire. In the morning he thought to repair this neglect by heating it to an unufual degree; but the fudden tranfition from cold to heat killed a great number of fine plants, and among others all the cinnamon trees from Martinico, of which the trunks were as thick as a man's arm, the heads very large, and of the greateft beauty; and alfo deftroyed the plants *Crefcentia*, *Achras*, *Annona*, *Portlandia*, and a *Coccoloba Grandifolia*, which was 20 feet high, and whofe leaves were of the fize of two feet.

The garden receives plants from other countries.

Maria Therefa patronifes the inftitution.

Many valuable plants deftroyed by the negligence of the attendants with regard to the hot houfe.

This garden alfo fuffered another lofs. A confiderable collection fent from the ifle of France by M. Gere, arrived at Triefte entirely fpoiled, the trees all dead, and the feeds improlific.

A cargo of plants from the Ifle of France fpoiled.

At this time the emperor, Jofeph the fecond, directed M. Jacquin and M. Born to propofe to fome men of abilities to undertake a voyage into remote countries. Profeffor Marter was appointed the conductor of this expedition, and Doctor Stupiez was affociated with him, together with the gardeners Boor and Bredemeyer, and the painter Moll. This company of travellers quitted Vienna in the month of April, 1783, and arrived in September at Philadelphia. They travelled over Pennfylvania,

Prof. Marter and others fent to collect in America in 1783.

Pennſylvania, Virginia and Carolina. M. Boor along with M. Schopf, made a journey into Florida, and from thence paſſed to the iſland of Providence. M. Bredemeyer returned from Carolina, and paſſing through England, arrived at Vienna in November, 1784, with ſeveral very beautiful plants. Boor, who during his ſtay at the Bahama iſlands had collected many rare plants, returned to Vienna in the month of September, 1785. But the painter Moll and Doctor Stupiez were ſeparated from their fellow travellers.

Several fine plants arrive at Vienna from thence, with M. Bredemeyer.

By the orders of the emperor, M. Bredemeyer, and the gardener Schucht, went towards the end of the year 1784 to rejoin the director of this expedition, M. Marter, who remained all this time in America; they paſſed over many of the great iſlands and a part of the continent as far as the mouth of the river Oronoco.

Bredemeyer ſent out again, —ſearches the iſlands and continent to the river Oronooko.

In 1788 they returned by way of Amſterdam to Vienna, and brought back many rare and new plants. M. Marter alſo arrived the ſame year, by the way of London and Bruſſels with a new collection of plants.

Many rare plants brought back by him and M. Marter in 1788.

The emperor had not forgotten the loſs of the plants from the iſle of France, and commiſſioned M. Boor and the gardener Scholl to go there, and touch on their way at the Cape of Good Hope. In the month of May 1786, they arrived at the Cape with the Dutch veſſels; M. Boor remained there till 1787, and then departed by himſelf for the iſle of France and that of Bourbon. In the month of January, 1788, he returned to the Cape with 280 caſes full of rare plants; and on the 20th of July in the ſame year arrived at Vienna with a great number of magnificent vegetables; but as all the caſes could not be brought in the veſſel, the gardener remained at the Cape with the remainder. There has not ſince been any poſſibility of getting them to Vienna, as well as many other plants; and the gardener Scholl remains at the Cape from that time, from whence he has ſent from time to time ſeeds and roots. Beſides this increaſe to the garden, the number of plants was augmented in different manners. Thus, at the ſale of the garden of Schwenk at the Hague, the emperor cauſed all the rare plants to be bought; and likewiſe M. Jacquin, the ſon, when he was on his travels over a great part of Europe, ſent to Schœnbrunn many exotic plants which he found in other gardens.

M. Boor and Scholl ſent to the Iſle of France.

M. Boor returns with many fine plants, —leaves ſome behind at the Cape with Scholl.

The plants of Schwenk bought.

M. Jacquin, the ſon, ſends many exotics from his travels.

The

The emperor Joseph also enlarged the hot-houses, and caused others to be built. In order to bring back Scholl the gardener to Vienna, with the plants which remained in his care at the Cape, the emperor Leopold, in 1791, ordered the gardener Bredemeyer and young Van der Schot, (the son of him who had been with M. Jacquin in the East Indies) to sail to the Isle of France, where Cere had collected many plants for the imperial garden, and during their return they were to touch at the Cape to take up all those which remained with Scholl. The captain of the vessel, in which the two gardeners had taken their passage put into Malaga; where they discovered in time that he had bad intentions with respect to them; which obliged them to return to Vienna without performing their commission. After the death of the emperor Leopold, his successor Francis the second had an hot-house constructed, 235 feet long, for the plants of the Cape. A new garden was also established, of which Doctor Host was appointed inspector, and in which were carefully cultivated all the plants which grew in the states belonging to the house of Austria.

Emperor Joseph enlarges the hot houses and builds others.

Emperor Leopold orders Bredemeyer to the Cape for Scholl,—he is disappointed of his passage.

Francis II. builds an hot house 255 feet long for Cape plants.

A new garden added for plants of the Austrian states.

By these details may be seen with what care this justly celebrated garden was augmented from the reign of Francis the first, and all astonishment will cease at the riches it contains, and which have furnished materials for different magnificent works on Botany, such as the *Icones plantarum rariorum*, published by M. Jacquin, and above all, that which appeared a few years ago, under the title of *Plantarum rariorum Horti Cæsarei Schœnbrunnensis descriptiones et Icones*, in two volumes folio, containing 150 coloured engravings.

Valuable Botanic publications from the garden of Schœnbrunn.

XI.

Letter from a Correspondent on the Means of increasing the Action of Sound on the Organs of such as are partially deaf.

To Mr. NICHOLSON.

SIR

ALTHOUGH I am so deaf as not to be able to hear the beating of a watch, unless it be put close to the ear, yet, if I place one end of a stick, or of a metal rod between my teeth and

Sounds imperfectly heard rendered audible through a solid applied to the teeth.

and the other end upon the watch, at the diftance of feveral feet, I can hear it very diftinctly.

The hearing confiderably affifted by preffing the external ear forward;

I know only two methods of alleviating the difficulty of hearing articulated founds; one is by furrounding the ear, with the hand open, and preffing it forward, the fingers and thumb being fixed behind it; this expedient does more than might be fuppofed. Another method is, that of the application of a trumpet; which however, is of but little ufe, conftructed as it is at prefent.

Or by a trumpet, though not confiderably ufeful.

The difcovery of any inftrument to facilitate hearing, by being placed in the mouth (probably after the manner of a tobacco-pipe) would be of great importance to a numerous clafs of our fellow creatures, whofe faculty of hearing is nearly fufficient for common converfation. If an inftrument fhould be invented, which will do any thing at all in this way, our experience in regard to other inventions, encourages an expectation, that improvements will follow: means of affifting human fight have long been devifed; little indeed has been done to affift defective hearing; it is however an object deferving of more attention than has been beftowed upon it. If you fhould be fo good as to infert this in your Journal, I indulge a hope, that fome of your ingenious correfpondents, compaffionating the unfortunate fituation of thofe whofe hearing is imperfect, may be led to attempt difcoveries, the refult of which may be of extenfive utility. It is defirable to afcertain the beft form and fize of an ear trumpet, and what metal is to be preferred.

Probability that an inftrument might be invented to act on the teeth or bones of the head fo as to magnify founds.

I am, Sir,

Your moft obedient Servant,

A. B.

Reference to fome difquifitions on founds in the quarto feries of this Journal.

P. S. On referring to my quarto edition of the Journal, vol. IV. page 383, I find fomething correfponding to my own obfervation. I fhall be extremely obliged if your humanity fhould determine you to infert the above, as it may be a means of exciting inveftigation on a fubject which is certainly of great confequence.

ANNOTATION—W. N.

Defultory remarks on the modification of found by means of folid bodies.

A CONSIDERABLE mafs of fpeculation concerning found, and the means of encreafing its action on the organs of fenfe is to be found in my annotations on the experiments of Perrole, at p. 416 of vol. I. of the Quarto Series of this Journal. The excellen

excellent papers of Mr. Gough, at page 66 and 160 of vol. X. of the prefent Octavo Series, concerning the augmentation of founds, and the fpeaking trumpet have added confiderably to our knowledge of this fubject. The memoir of Haffenfratz on the fame inftrument in our Ninth Volume, p. 283, and another, by the fame author, on the Propagation of Sound, at Vol. XI. p. 127, alfo deferve to be confulted. From the whole confideration of the facts it feems as if the fonorous vibration of the inftrument were of much more confequence than has hitherto been fufpected; and it feems not improbable, that a large furface expofed to the aerial pulfes of found, and having a tail of communication to be applied to the teeth, or inferted in the ear, might have confiderable effect. The ufe of the external ear, which has excited fo much difcuffion, may, perhaps, be of this kind. The experiment of Dr. Moyes (Philos. Journal, III. 57) of conveying found to great diftance through a ftring may be added to the other facts; and tends to fhew that the fonorous undulation does not require to be tranfmitted through fuch bodies as are the moft denfe, uniform, and elaftic. Leather, or felt, or pafteboard, or various other fimilar materials, are more frequently obferved to tremble in the hand at certain particular founds than many other denfe bodies.

XII.

Eafy and Correct Method of verifying the Portion of a Tranfit Inftrument. By J. S. BUTT, *Efq. Communicated by the Author.*

To Mr. NICHOLSON.

SIR, *Paragon, December* 8, 1805.

A SHORT note having appeared in Mr. Kelly's new edition of Spherics, defcribing my method of verifying the pofition of a tranfit inftrument, and thinking an account more in detail may not be unacceptable to your aftronomical readers, I am induced to trouble you with it, that you may, if you pleafe, infert it in your valuable Journal. Introductory note.

Your's refpectfully,

JAMES STRODE BUTT.

TO

The uſual method of adjuſting a tranſit inſtrument by a circumpolar ſtar.

TO make the line of collimation move in the plane of the meridian, we are deſired to obſerve the tranſits of circumpolar ſtars, and if the intervals between the times of their tranſits are equal, the tranſit inſtrument moves in the plane of the meridian: for the axis and line of collimation being previouſly adjuſted, it muſt paſs through the zenith; and if it divides the circle deſcribed by any circumpolar ſtar, into two equal parts, it muſt paſs through the pole.

— requires the clock to keep time for at leaſt 24 hours.

But here a difficulty ariſes which is a probable alteration, or a want of uniformity in the rates of the clock or watch for ſo long a period as twenty-four hours, or during that portion of time which the obſerver may require to repeat his obſervations, ſo as to be ſatisfied.

Method independant of this rate and of previous right aſcenſion, &c.

A method independant of the rate of a clock or watch for ſo long a time, and alſo entirely of any other previous obſervations of right aſcenſion, is a deſideratum to practical aſtronomers, and alſo to thoſe who occaſionally amuſe themſelves by obſerving time, and the rates of their chronometers, in their preſent improved ſtate; but who may be unacquainted with aſtronomical equations, of preceſſion, nutation, &c.

Rule, obſerve the tranſits of two different ſtars, one above and the other below the pole; which differ only a ſhort time, for example, only a few minutes: at any time afterwards repeat the obſervation upon the ſtars when their ſituations as to the pole are reverſed. If the difference in time be the ſame the tranſit is duly placed, if not it muſt be altered, &c.

Rule, Obſerve the difference of tranſits of any two circumpolar ſtars, that are ſituated nearly in the ſame azimuth, or vertical circle, the one above and the other below the pole: and whoſe difference of right aſcenſion is nearly 180°; (namely)

Obſerve, The tranſit of α caſſiop. above the pole, and immediately after it the tranſit of ε urſæ majoris below the pole, whoſe difference of tranſit is not more than 15 minutes, and for ſo ſhort a time the clock or watch may be ſafely depended on. Then invert the operation, and obſerve the tranſit of α caſſiop. below, and ε urſæ above the pole. If their difference of tranſits is the ſame in both obſervations, the tranſit inſtrument is accurately in the meridian; if not the error may be corrected by altering the poſition of the inſtrument till their difference of tranſit is the ſame in both obſervations.

Should the error be great it may be corrected nearly by any of the theorems now in uſe; (*vide* Wales on Time-Keepers) or half the difference may be ſubſtituted for the error, and by repeated approximation the tranſit inſtrument may be accurately adjuſted.

The

The advantage of this method is, that you rely upon the stars keeping $\frac{99}{100}$ of the time, which would otherwise be kept by the clock or watch; and it is of no consequence whether the observations follow one another on the same day or week, provided the instrument is adjusted to the same point of the horizon previous observation, for there is little or no difference in their precession, &c. during an interval of a month.

Advantages. Most of the time is kept by the stars, and not by the clock.

Another advantage is, that the observations follow each other so soon, that you are not likely to be disappointed by a change of weather, for each pair of observations is complete as far as it goes, which is not the case in the other method, which requiring an interval of twelve hours between each observation, a change of weather is more likely to take place.

The short interval insures against change of weather; &c.

A transit instrument is the basis of astronomy, and whoever has the fixing of it should consider himself independant of every previous observation, and acting entirely upon principle, which is not the case where the adjustment is by previously observed right ascension, and which require reducing to the day of observation; indeed nicely reduced right ascensions are not always in the hands of those who may be wish to be in possession of a simple and accurate method of placing a transit instrument precisely in the meridian.

Other useful remarks.

This method was devised and used by me since 1794, but I have never read or heard of any one using the same.

J. S. B.

N. B. Proper stars in this Lat. are,

α Cass. and ϵ Ursæ Majoris.
β Cass. and δ Ursæ.
γ Cass. and ϵ Ursæ.

Also the stars of Draco and Auriga,
Cepheus and Ursa,
Perseus and Draco,

A large comet was discovered at the Royal Observatory Dec. 8, which passed the meridian at 6.^{h} 24.^{m} 7. mean time.

A large comet.

Observed right ascension was - 353° 6′ 41″
Declination south - - - - - - 23° 41′ 8″

*** I have since heard that this comet was not again seen, but is supposed to have proceeded southward.—N.

A Com-

XIII.

A Compariſon of ſome Obſervations on the Diurnal Variations of the Barometer, made in Peyrouſe's Voyage round the World, with thoſe made at Calcutta by Dr. Balfour *.

Barometrical obſervations between the Tropics by Lamanon and by Dr. Balfour.

THE firſt of the obſervations here referred to were made by M. Lamanon, an ingenious naturaliſt who accompanied Peyrouſe, and who has given an account of them, (ſee fourth volume of the *Voyage*, octavo edit.), in a letter to M. de Condorcet, dated St. Catharine, 5th November 1785. Dr. Balfour's Obſervations are in the *Aſiatic Reſearches* for 1794, and a ſhort account of them is alſo inſerted in the fourth volume of the *Tranſactions, R. S. Edin.* Hiſt. p. 23.

M. Lamanon's obſervations were made in conſequence of inſtructions from the Academy of Sciences, directing him to keep an exact account of the heights of the barometer in the vicinity of the equator at different hours of the day, with a view to diſcover, if poſſible, the quantity of the variation of that inſtrument, due to the action of the ſun and moon, that quantity being there probably as its *maximum*, while the variations ariſing from other cauſes are at their *minimum*.

Lamanon uſed a marine Barometer of Nairne.

M. Lamanon was provided with one of Nairne's marine barometers, which, he ſays, was ſo little affected by the motion of the ſhip, that it might be depended on to the $\frac{1}{100}$ of an inch. In this barometer, he tells us, that from about the 11th degree of north latitude, he began to perceive a certain regular motion, ſo that the mercury ſtood higheſt about the middle of the day, from which time it deſcended till the evening, and roſe again during the night. As they approached the equator, this became more diſtinctly perceptible; and on the 28th of September, the ſhip being then 1° 17′ north latitude, a ſeries of obſervations was begun, and continued for every hour till the 1ſt of October, at 6 A. M. The following abſtract ſhews the reſult of the obſervations on the 28th and 29th.

Regular diurnal change in lower Lats. than 11°. N.

Twenty eighth of September.

From 4 to 10 A. M.	Barometer roſe	1*l*.	$\frac{9}{10}$
From 10 A. M. to 4 P. M.	fell	1	$\frac{2}{10}$
From 4 to 10 P. M.	roſe	0	$\frac{9}{10}$

* From the Hiſtory of the Royal Society of Edinburgh, 1805.

Twenty

Twenty ninth of September.

From 10 (28th) to 4 A. M.	fell	1*l*. $\frac{3}{10}$
From 4 to 10 A. M.	rofe	1 $\frac{5}{10}$
From 10 A. M. to 4 P. M	fell	1 $\frac{3}{10}$
From 4 to 10 P. M.	rofe	1

The obfervation on the 30th were to the fame effect; and hence it is concluded that at the equator the flux and reflux of the atmofphere produces in the barometer a variation of about 1 line $\frac{2}{10}$ Englifh, correfponding, as M. Lamanon remarks, to a height in the atmofphere of nearly 100 feet. According to Bernouilli, the action of the fun and moon fhould produce a tide of feven feet, and according to Mr. de la Place, a tide not nearly fo great.

The effect is greater than might arife from the computed atmofpheric tides.

It fhould be obferved, that when thefe obfervations were made, the moon was in her laft quarter, and the fun a few degrees to the fouth of the equator. The latitude on the 28th was 50′ north, and 11′ north on the 29th; in the night between that and the 30th, the fhip croffed the line; and on the 30th at noon, the latitude was 42′ fouth: the longitude all this while between 17° 31′ and 18° 33′ weft of Paris, by the time-keeper; fo that the coaft of Africa, which was the neareft land, was diftant about 8° of a great circle, and the American continent about 19°.

Situation of the fun and moon, and place of the fhip. She was *far out at fea*.

The agreement between thefe, and Dr. Balfour's obfervations at Calcutta is very remarkable. Dr. Balfour found that during the whole lunation, in which he obferved the barometer from half-hour to half-hour, the mercury conftantly fell from 10 at night to 6 in the morning; from 6 to 10 in the morning it rofe; from 10 in the morning to 6 at night it fell again; and laftly rofe from 6 to 10 at night. The *maximum* height is therefore at 10 at night and 10 in the morning, and the *minimum* at 6 at night and 6 in the morning. The only difference is, that in Mr. Lamanon's obfervations, the *minimum* is ftated to have happened about 4 inftead of 6. This, however, will not feem a very material difference, when it is remembered, that the inftant when any quantity attains either its greateft or its leaft ftate is not eafily afcertained with precifion. From the obfervations as detailed by M. Lamanon, the time of the *minimum* feems to anfwer fully as well to 5 as to 4; fo that the difference of the refults is in

Argreement between thefe Obfervations, and thofe of Dr. Balfour *at Calcutta*.

every

every view inconfiderable, and their coincidence on the whole, not a little fingular. The variations in Dr. Balfour's barometer between the neareft *maximum* and *maximum* is fometimes about $\frac{1}{10}$ of an inch, though in general confiderably lefs.

Whether the caufe which produces land and fea winds could produce the regular change.

In the abftract of Dr. Balfour's obfervations referred to above, it is remarked, that it feems not improbable that thefe variations of the barometer are connected with the reciprocations of the fea and land winds during the day and night. But whatever may have been formerly the probability of this fuppofition, it is entirely deftroyed by the obfervations of the French navigators. Thefe obfervations were made too far out at fea to leave room for fuppofing that the land winds had any influence on the phenomena to which they refer. It is at the fame time doubtful, whether thofe phenomena can be afcribed to the atmofpherical tides produced by the fun and moon, as the *ebbing* and *flowing* of the mercury in the barometer appears to have no dependence on the pofition of thofe luminaries relatively to one another, but happens, it would feem, conftantly at the fame hour, in all afpects of the moon and all feafons of the year. The fubject is well deferving of a fuller inveftigation. We fhould probably before now have had farther information refpecting it, if happily the able navigator above-named, and his brave affociates, had been deftined to revifit their native fhores. The cruel fate of an expedition fo well planned, and fo well appointed for the purpofes of fcience, will never ceafe to be matter of fincere regret.

—moft probably not.

—neither is it likely that it was caufed by tides in the air, as it does not follow the moon.

Annotation.—W. N.

Probability that the equi-tropical change is caufed by afcending and defcending currents in the atmofphere.

I have inferted the foregoing with a view, in fome meafure, to afford a comparifon with Mr. Horfburgh's paper on the fame fubject, at page 16. It is not without diffidence that I venture to propofe a conjecture on this fubject, which in fact requires more confideration than I can, at prefent, beftow on it. Its change feems to me to be governed by the afcent of the air which would take place immediately beneath the fun, if the earth were ftationary, and the furrounding defcent of the fame fluid, of which the circumftances and modifications are fo well explained

plained by Prieur in his memoir on the morning and evening dew (at p. 86, vol. IV. of our quarto feries.) The confiderations there detailed may be eafily extended to fhew alfo that the effects muft be greatly altered, and, in moft inftances, obliterated by the vicinity of land; which even changes the regular trade winds into land and fea breezes.

XIV.

Abftract of a Memoir on the Direction and Velocity of the Motion of the Sun and Solar Syftem. By Dr. HERSCHEL. *From the Philofophical Tranfactions,* 1805. (A.)

Proper motions of the fixed ftars.

THE learned author begins his paper by noticing Dr. Mafkelyne's table of the proper motions of 36 ftars of the firft magnitude, and conceives that if this table affords proof of motion in ftars in our immediate neighbourhood, the changes of pofition in minute double ftars, many of which are only to be feen by means of the beft telefcopes, likewife prove that motions are equally carried on in the remoteft regions of fpace.

Deduction of a proper motion of the fun and its direction.

In 1783, the Doctor deduced from the proper motions of the ftars, a motion of the fun and folar fyftem towards λ hercules; and the opinion he then conceived has been much ftrengthened by the confiderations ftated in the following pages. Should this doctrine be eftablifhed, many phenomena may be accounted for, which without it muft remain inexplicable.

Confiderations in favour of fuch a motion.

Though it was propofed, by the admiffion of a folar motion, to take away many of the proper motions of ftars, by invefting the fun with a contrary one; our author admits that it will reveal a vaftly greater number of concealed real motions than would be neceffary to admit, were the fun at reft; and that the neceffity for admitting its motion ought therefore to be well eftablifhed.

Reafons from analogy of the planets, &c.

The motion of fatellites round their primary planets, and of thefe round the fun, fuggefts the idea of a revolution of the latter round fome other unknown centre; nor are we without hypothefes built upon this conjecture.*

The poffibility of a folar motion has been fhown by the late

* See Syftême du Monde de Lambert, p. 152, 158. Alfo Phil. Tranf. for the year 1783, p. 283.

Dr.

Dr. Wilſon, of Glaſgow, upon theoretical principles; and its probability, from reaſons of the ſame nature, by M. de Lalande.

Probability that the ſun has progreſſive as well as rotatory motion,

The rotatory motion of the ſun, from which the latter concludes a diſplacing of the ſolar centre, indicates a motion of tranſlation in ſpace; for it is not very probable that the mechanical impreſſion which gave the former, ſhould not alſo occaſion the latter. This however can be admitted only as a plauſible hypotheſis, until we attain a knowledge of the cauſe of the rotatory motion.

—and the variable ſtars alſo.

This argument might be ſtrengthened by cloſely obſerving the ſtars which change their magnitudes periodically; for if theſe changes ariſe from a rotatory motion,* a real motion in ſpace may be expected to attend it; and the multitude of theſe ſtars is ſo great, that their concurrent teſtimony is deſirable.

Three ſorts of motions of ſtars.

But ſetting aſide theoretical arguments, the Doctor notices that as all parallactic motions indicate the obſerver not to be at reſt, it may be neceſſary to explain three ſorts of motions, which will frequently be alluded to in the following diſcuſſion.

Parallactic, real, and apparent.

Suppoſe the ſolar ſyſtem to move towards a certain part of the heavens, the ſtars, to an inhabitant of the earth, will appear to move in an oppoſite direction. Let $s\,p$ (*Pl.* II. *Fig.* 1.) repreſent the parallactic motion of a ſtar; which, if the ſtar have no real motion, will alſo be its apparent motion; but if it ſhould have a real motion, which in the ſame time that it could have gone from s to p, would have carried it from s to r, it will be ſeen to move along the diagonal $s\,a$; and $p\,a$, being parallel and equal to $s\,r$ will repreſent its real motion. The triangle $s\,p\,a$ is ſuppoſed to be formed in the concave of the heavens by three arches of great circles, the obſerver being in the centre, and $s\,p$ repreſents the parallactic, $p\,a$ the real, and $s\,a$ the apparent motion of the ſtar. The ſituation and length of theſe arches in ſeconds of a degree will repreſent the direction and quantity of each motion; and calling the ſolar motion S, the diſtance of the ſtar from the ſun d, and the ſine of the ſtar's diſtance from the point towards which the ſun is moving ϕ; the parallactic motion will be expreſſed thus: $\frac{\phi . S}{r . d} = s\,p$.

The largeſt ſtars are moſt fit to ſhew the ſun's motion.

A motion of the ſun will occaſion parallactic motions of the ſtars, and *vice verſa*; but to aſcertain if parallactic motions exiſt, ſuch ſtars ſhould be examined as are moſt viſibly affected

* See Phil. Tranſ. for the year 1795, p. 68, and our Journal, XI. 271.

lar motion; which points out the brightest stars as most proper for the purpose; for any star may have great real motion, but to have great parallactic motion it must be in the neighbourhood of the sun.

Parallactic may be distinguished from real motions by their directions: for, if a solar motion exist, all parallactic ones will tend to a point in opposition to the direction of that motion; but real motions will be indiscriminately dispersed. The parallactic motions are directed to a point,

Under these distinctions, the proper motions of the stars, if the sun be not at rest, will be parallactic, or composed of real and parallactic; the latter case constituting the apparent motion of the star. and will combine with the real (angular).

Dr. H. next describes the meeting of the arches arising from a calculation of the proper motions of the 36 stars in Dr. Maskelyne's catalogue on a celestial globe, of which ten were made by stars of the first magnitude, about the constellation Hercules; beyond these there was no appearance of any other than a promiscuous situation of intersections.—Of the intersecting points, that towards which the sun moves is denominated the apex of its motion; and as the stars will then have a parallactic motion towards the opposite point, it has received the appellation of a parallactic centre. Deduction of the parallactic centre from obs. on many stars.

Tabulated results.

Intersecting points.	Right Ascension.	Polar distance North.
	° ′ ″	° ′ ″
1. Sirius and Arcturus, in the mouth of the Dragon - - -	255 39 50	36 41 34
2. Sirius and Capella, near the following hand of Hercules - -	275 9 32	64 21 48
3. Sirius and Lyra, between the hand and knee of Hercules - -	272 23 58	58 23 24
4. Sirius and Aldebaran, in the following leg of Hercules - -	263 25 38	44 39 47
5. Arcturus and Capella, N. of the preceding wing of the Swan -	290 0 58	32 7 23
6. Arcturus and Aldebaran, in the neck of the Dragon - - -	267 2 19	33 57 20
7. Arcturus and Procyon, in the preceding foot of Hercules -	235 3 13	46 21 34
8. Capella and Procyon, S. of the following hand of Hercules -	272 51 49	73 7 56
9. Lyra and Procyon, preceding the following shoulder of Hercules	266 46 49	66 48 11
10. Aldebaran and Procyon, in the breast of Hercules - -	260 1 29	60 59 34

As

Confirmation by other ſtars.

As a further confirmation that the parallactic motion may be perceived in the motion of the brighteſt ſtars, Dr. H. examined the interſections made by the proper motions of ſome large ſtars of the next order, with the arches in which the ſtars of the firſt magnitude move, and found 15 which gave ſimilar reſults with the former 10, in pointing out the ſame part of the heavens as a parallactic centre.

This reſult confirmed by double ſtars.

Changes in the poſition of double ſtars indicate the ſame reſult, and may therefore be more eligibly aſcribed to the effect of parallax, than to admit of ſeparate motions in different ſtars: for, if the alterations of the angle of poſition were owing to a motion of the largeſt ſtar in each ſet, ſuch motions muſt, in contradiction to probability, tend nearly to one particular part of the heavens. This argument derives its validity from the ſame ſource with the former, viz. the parallactic motions of at leaſt 28 more ſtars pointing out the ſame apex of a ſolar motion, by their direction to its oppoſite parallactic centre.

and by the harmony of the proper motions deduced from it.

The incongruous mixture of great velocity and extreme ſlowneſs in the proper motions of the ſtars of the ſame magnitude, is removed by the conſideration of parallax from the ſolar motion; and it will be ſeen that there is a general conſiſtency in their motions. The ſame obſervation is alſo applicable with reſpect to the ſidereal occultation of a ſmall ſtar in the Swan.

Inveſtigation of the direction of the ſun's proper motion.

Dr. H. concludes from the foregoing premiſes, that the expediency of admitting a ſolar motion will not be queſtioned, and proceeds to inveſtigate its direction. He begins by proving, that when the proper motions of two ſtars are given, an apex may be found, towards which if the ſun be ſuppoſed to move with a certain velocity, the two given motions may be reſolved into apparent changes ariſing from ſidereal parallax, the ſtars remaining perfectly at reſt. For we muſt not admit more motions than are ſufficient to account for the obſerved changes in the ſituation of the ſtars; and it would be wrong to have recourſe to the motions of two ſtars, when that of the ſun alone may be ſufficient to account for both; which conſideration would be a ſufficient inducement for fixing at once on the calculated apex as well as on the relative diſtances aſſigned to the two ſtars, could other proper motions be, with equal facility, reſolved into ſimilar parallactic appearances: but, when a third ſtar does not direct towards the

An apex or parallactic center is deduced from the apparent motions of two ſtars, ſuppoſed to have no real motion.

5 ſame

fame apex as the former two, its apparent motion cannot be refolved by the effect of parallax alone; and this difficulty is further enhanced by the number of apices required to folve all proper motions into parallactic ones, increafing, not as the number of ftars admitted to have proper motions, but, when their fituation is favourable, as the fum of an arithmetical feries of numbers, beginning at 0, continued to as many terms as there are ftars admitted.

The author here propofes an illuftration of his fubject by confidering the three apices, or interfecting points, No. 1, 2, 5, in the foregoing table.

The diftance of Arcturus from the apex of the folar motion is found to be 47° 7′ 6″, and its parallactic motion, which is as the fine of that diftance 2.08718″, which is the apparent motion of Arcturus, as eftablifhed by obfervation. Namely Arcturus and Sirius.

Admitting Sirius to be a very large ftar, at the diftance of 1.6809 from us, and computing its elongation from the apex of the folar motion at 138° 50′ 14.5″, its parallactic motion will be $\frac{\odot . S}{r . d} = s\,p = 1.11528''$, which alfo agrees with the apparent motion already afcertained by obfervation as the proper motion of Sirius.

The diftance of Capella from the apex of the folar motion is 80° 51′ 46″, and admitting the velocity of the fun towards the before given point, it will occafion a parallactic motion of Capella, in a direction 89° 51′ 18″ fouth-following its parallel, amounting to 2.8125″. Capella is here taken for a ftar of the firft magnitude, fuppofing its diftance from us to be equal to that of Arcturus. Hence the parallactic motion of another, viz. Capella, is deduced:

By conftructing a triangle, the fides of which reprefent the three motions of every ftar, not at reft; one of the fides, reprefenting the apparent motion, will be equal to 0.4637″; the other fide, being the parallactic motion, 2.8125″; and the included angle 18° 19′ 27″, from which will be obtained the third fide, or the real motion of the ftar, 2.3757″. By the given fituation of this triangle with refpect to the parallel of declination of Capella, the angle of the real motion will be had, which is 86° 34′ 11″ north-following the parallel of this ftar. A compofition of the parallactic and real motions in the directions, will produce the annual apparent motion, as eftablifhed by obfervation. and by refolving this into the apparent motion and another, this laft will be the real (angular) motion, (fuppofing the other ftars to have none.)

It is here obferved, that although the proper motion of a third

But it is not fit that all the real motion should be ascribed to Capella;

third star is accounted for by retaining the same apex of the solar motion which explained the apparent motions of the other two, yet a great degree of real motion has been assigned to Capella, of which Arcturus and Sirius have been altogether deprived; which shews that the apex of solar motion must be so fixed as to be equally favourable to every star proper for directing our choice. Hence a problem arises, for discovering a point whose situation among three given apices shall be such as if the sun's motion be directed towards it, there may be taken away the greatest possible quantity of proper motion from the three given stars. The intricacy of this problem is, that by a change of the distance of the apex from any one of the stars, its parallactic motion, which is as the sine of that distance, will be affected; so that it is not merely the alteration of the angle of direction which is concerned. From the solution of this problem, a much more complex one would arise, as three stars would certainly not be sufficient to direct the present endeavour to find the best situation of an apex for the solar motion.

but the apex must be taken so as to leave the real motions as small as possible.

Apparent motions of six bright stars

It was before shewn that the brightest stars are the most proper for demonstrating the effect of parallax, and that in searching after the direction of the solar motion, the aim should be to reduce the proper motions of the stars to their lowest quantities. The six principal stars, whose intersecting arches have been given, when their proper motions in right ascension and polar distance are brought into one direction, will have the following apparent motions:

tabulated.

Names of the Stars.	Direction of the apparent Motions.				Quantities of the apparent Motions, per Year
Sirius,	68°	49′	40.7″	South-preceding.	1.11528″
Arcturus,	55	29	42.0	Ditto.	2.08718
Capella,	71	35	22.4	South-following.	0.46374
Lyra,	56	20	57.3	North-following.	0.32435
Aldebaran,	76	29	37.3	South-following.	0.12341
Procyon,	50	2	24.5	South-preceding.	1.23941
				Sum of the apparent Motions,	5.35337

In

In seeking a solar motion, which requires the least motion in the above six stars, let the line *p a*, *Fig.* 1, which represents the real motion, be brought into the situation *m a*, and the real motion required will then be at a minimum. If by the choice of an apex for the solar motion the angle at *s*, made by the lines *s p* and *s a*, can be lessened, the quantity of real motion required to bring the star from the parallactic line *s p m* to the observed position *a*, will also be diminished.

Deduction of a solar motion that shall leave the real motions of these the least possible.

It has already been shewn that when two stars only are given, the line *s p* may be made to coincide with the line *s a* of both stars, whereby their real motions are reduced to nothing; and that when three stars are concerned some real motion must be admitted in one of them. Now, since all parallactic motions are directed towards the same center, a single line may represent the direction of the effect of the parallax. Therefore, let *s* P or *s* S, *Fig.* 2, stand for the direction of the parallactic motion of the stars; and as in the foregoing table we have the angles of the apparent motion of six stars, with the parallel of each, the direction of the line *s* P or *s* S must be computed with the parallels of the same stars, which may be done as soon as an apex for the solar motion is fixed upon. The difference between these angles and the former will give the several parallactic angles P *s a* or S *s a*, required for an investigation of the least quantity, *m a*, belonging to every star.

A single line may shew the direction of parallactic effect in two stars, &c.

The author exemplifies what he here lays down, by supposing the sun to move towards λ Herculis; and calculating the required angles of the direction in which the effect of parallax will be exerted with the six stars already selected, he obtains the angles of the parallactic motion with the parallel, the difference between which and the former apparent angles with the parallel of each star gives the angles of the apparent with the parallactic motion, as represented in *Fig.* 2. The lines *s a* represent the annual quantity of the apparent motions.

Computation, supposing the sun to move towards λ Herculis.

When the situation of the last mentioned angles is regulated as in the figure alluded to, the several lines *m a* may be drawn perpendicular to S P, and by computation their quantity will be found to be—

Sirius	-	-	0.65427′
Arcturus	-	-	1.28784
Capella	-	-	0.10887
Lyra -	-	-	0.11281
Aldebaran	-	-	0.01404
Procyon	-	-	0.04998
Sum			2.22491

The refult of this inveftigation is, that by admitting a motion of the fun towards λ Herculis, the annual proper motions of the fix ftars alluded to, of which the fum is 5.3537″, may be reduced to real motions of no more than 2.2249″.

A more favourable apex. The author here obferves, that although the precife place of the beft apex is difficult to afcertain, a more favourable one than that above propofed may be obtained: for, by infpection of the figure which reprefents the quantities of real motion required, when λ Herculis is fixed upon, it will appear that by a regular method of approximation, the line S P may be turned into a fituation, wherein all the angles of the apparent motion of the fix ftars will be much reduced; and it is evident that the parallactic line SP fhould be turned more towards the line *sa*, reprefenting the apparent motion of Sirius. He accordingly tries a point near the following knee of Hercules, whofe right afcenfion in 270°. 15′. and north polar

Its fituation. diftance 54°. 45′, fee *Fig.* 3, the quantities required for conftructing which are found by the fame method as already defcribed in *Fig.* 2. By a calculation of the angles and the leaft quantities of real motion, according to this apex, it appeared that the annual motion of the fix ftars was reduced to 1.4594″, which is 0.7655″ lefs than when the apex was λ Herculis.

Suppofition that Sirius may be moft affected by parallax, as brighteft; In the approximation to this point, it appeared, that when the line of the parallactic motion of Sirius was made to coincide with its apparent motion, a certain minimum might be eafily obtained of the other parallactic motions. But as Sirius has not the greateft proper motion, the author conceived that another minimum, obtained from the line wherein Arcturus appears to move might be more accurate; as this ftar from its great proper motion may be more affected by the parallax

or Arcturus, as having the greateft apparent motion. arifing from the motion of the fun. He therefore chofe a point not only in the line of the apparent motion of Arcturus, but equally favourable to Sirius and Procyon, the remaining two ftars which have the greateft motions.

"H

"If the principle of determining the direction of the solar motion by the stars which have the greatest proper motion, be admitted", observes the author, "the following apex must be extremely near the truth: for, an alteration of a few minutes in right ascension or polar distance either way, will immediately increase the required real motions of our stars. Its place is, right ascension 245° 52′ 30″, and north polar distance 40° 22′". The calculation is delineated in *Fig.* 4. The sum of the least quantities of real motion in this experiment is 0.95595″, less than the former by 0.50843″. Apex on this last supposition.

In these calculations the author has proceeded upon the principle of obtaining the least possible quantity of real motion to ascertain the most favourable situation for a solar apex, and has proved that the sum of the observed proper motions of the six principal stars may be the result of a composition of two other motions; and that if the real motions were reduced to their smallest possible quantities, they would not exceed 0.9559.

The Doctor, however, seems to think that these real motions may not be brought down to the low quantities mentioned; and proceeds to shew that this circumstance will not affect the arguments he has used for establishing the method he has adopted; for, although the great proper motions of Arcturus, Procyon, and Sirius, are strong indications of their being affected by parallax, it is not probable that the apparent changes of their situations should be entirely owing to solar motion; but that their own real motions would have a great share in them; and it is evident that in parallactic motions the distance of a star from the sun is of material consequence; and as this cannot be assumed at pleasure, we are not at liberty to make the parallactic motion sp, *Fig.* 1, equal to the line sm; hence it follows that the real motion of the star cannot be from m to a, but will be from p to a. If, however, ma be a minimum, pa when sp is given, will also be a minimum, and if all the ma's in *Fig.* 4 be minima, the sp's will give the pa's as small as possible; which is the point desired to be established. If the nearest stars be most affected by parallax, their proper motions may also be more evident.

In concluding Dr. H. observes, that as it is known that proper motions do exist, and as no solar motion can resolve them entirely into parallactic ones, we ought to prefer that direction of the motion of the sun which will take away most real motion, and this, as has been shewn, will be done when the right ascension of the Apex is 245° 52′ 30″, and its north polar distance 40° 22′. Conclusion. Preferable situation of the apex.

XV.

New Experiments on the Solution of Sulphur in Alcohol, and the various Kinds of Ether. *By* M. FAVRE *.

Probability that ether would dissolve more sulphur than alcohol.

IN my first note on the solution of sulphur in alcohol, I announced an intention of examining the solvent power of the several ethers upon this combustible; which I had at that time been prevented from, by being obliged to leave Paris for Brussells, to take the office of apothecary to the military hospital. In the paper alluded to, I hinted that it was probable ethers would dissolve sulphur in greater quantity than alcohol; I had been led into this opinion by the results obtained from mixing this mineral with alcohol, at various degrees. I observed, as already stated, that the more alcohol was rectified, the more readily it dissolved sulphur; and *vice versa*, which difference I imagined to proceed from the greater quantum of hydrogen contained in highly rectified alcohol. Knowing ethers to contain less carbon and more hydrogen than alcohol, I had no doubt that they would dissolve a greater quantity of sulphur. The result of the several experiments, which I made under this impression, I am now about to detail: from which it will be perceived that I was not mistaken in my conjecture. I shall also subjoin the new experiments, which I made with alcohol, to ascertain the precise quanity of sulphur it is capable of dissolving, in order to compare the results with those obtained from ethers.

Preparation, &c. of the ethers.

The ethers I employed were prepared with much exactness, and according to the methods recommended by professor Fourcroy. I took care, in each experiment, to ascertain the specific gravity of the ether made use of, the quantity of sulphur dissolved by it, the various results obtained with or without the contact of the sun's rays, and the properties of sulphurated ether.

First and second Experiments.

Sulphuric ether by long digestion without heat took up nearly

In each of two six-ounce matrasses I put two drachms of the flowers of sulphur, prepared in the same manner as for

* Van Mons's Journ. de Chimie, Vol. VI.

the

the experiments mentioned in my first note, viz. nicely washed, and one ounce of rectified sulphuric ether, whose weight was 0.7396. Having secured the mouths of the mattrasses with luting, I put one in a very light place, and the other in a dark place. I shook them every day, and at the end of a month, filtered their contents. On examination the two sulphurated ethers obtained by these operations, presented the following characteristics:

one-thirteenth part of sulphur in the light; and only one-seventeenth in the dark.

The colour of the ether exposed to the light was scarcely changed; it had a powerful hydro-sulphurous smell, and its taste was disagreeable, and likewise hydro-sulphurous. Mixed with distilled water, it precipitated nothing; but I remarked that the water dissolved a much less quantity of it than when pure. In proportion as the ether became volatilised, the sulphur formed a whitish scum on the surface of the liquid, which at length was precipitated to the bottom of the glass in which the experiment was made. (I shall hereafter mention the quantity of this precipitate.) Put in contact with white metals, it deeply blackened them. (Care must be taken in this latter experiment to close exactly the mouth of the vessel in which the metals are placed in contact with sulphuric ether, on account of the great tendency of ether to be converted into gas by its attraction of caloric from surrounding bodies.) When mixed with a solution of acetite of lead, it gave a pretty considerable black precipitate.

The sulphurated ether prepared without light, possessed all the properties of the other, but in a less degree. It also was less impregnated with sulphur: for, on a repetition of the experiment, and carefully weighing the products, I found that each ounce of the ether prepared in the light contained 38 grains of sulphur; whilst that prepared in the dark held only 29.

Third and fourth Experiments.

Having proceeded as above described, with nitric ether weighing 0.9088, I obtained an ether whose colour was in no degree changed; its smell and taste, though hydro-sulphurous, were not so powerful as those of sulphurated sulphuric ether; mixed with distilled water, it presented the same phenomena, but deposited a less quantity of sulphur. It discoloured white metals less forcibly than the preceding ether;

Nitric ether by the same treatment took up nearly one twenty-second part of sulphur in the light; and only twenty-fourth in the dark.

ether; and, in a word, it had all the qualities of sulphurated sulphuric ether, but in a lower degree. It likewise contained a less quantity of sulphur; the result of the experiment made in the light being but 22 grains of precipitated sulphur; and 20 for that conducted in darkness.

Fifth and sixth Experiments.

Muriatic ether took up one thirty-seventh in the light; and only one fifty-third in the dark.

With muriatic ether, weighing 0.7196, proceeding as already described, and at the same proportions, I obtained a sulphurated muriatic ether, possessing all the peculiarities above mentioned, but weaker. It contained only 13 grains of sulphur, when conducted in the light, and $9\frac{1}{8}$ grains when managed in the dark.

Seventh and eighth Experiments.

Acetic ether took up very little sulphur.

Acetic ether weighing 0.8664, dissolved but a very small portion of sulphur, and its qualities were but slightly marked. It contained but three grains of sulphur in an ounce of ether, in the experiment made in the light, and about $1\frac{1}{2}$ grains in that made in the dark.

Ninth Experiment.

Solution of sulphur in alcohol was less charged than that of sulphuric ether.

Having made the foregoing experiments, I wished to ascertain the difference existing between the several ethers and alcohol, in respect to their capacity for dissolving sulphur: I therefore retraced the experiments I had formerly made with alcohol. To avoid the repetition of what has been already communicated in my first essay, I shall here merely state the quantity of sulphur I have been able to dissolve, either by submitting the mixture to a heat less than sufficient to cause the alcohol to boil, or by exposing it to the light, or by placing it in a dark place. For these experiments I used alcohol of 43 degrees.

After digesting for 12 hours over a gentle fire an ounce of alcohol with two drachms of the flowers of sulphur, I obtained 23 grains of precipitate.

Tenth and eleventh Experiments.

On leaving similar mixtures, one exposed to the rays of the sun, and the other in a place impervious to the light, during a month, and proceeding as already described, I obtained

tained 16 grains from the first mixture, and 13 from the second.

After what has here been laid down, it is evident that sulphuric ether dissolved the greatest quantity of sulphur; for, after frequently repeating the experiment, I found the average to be 25 grains in an ounce. Nitric ether and alcohol at 43 degrees, dissolved nearly in the same proportions; and acetic ether the least of any. Recapitulation.

It has been long a desideratum in medicine to discover a method of administering sulphur in a state of extreme division, especially in complaints of the lungs and diseases of the skin. With this intent, physicians have recommended it to be dissolved in essential oils, and to form what is known in pharmacy under the title of *balsams of sulphur, terebinthinated, anisated, &c.* These medicaments have the disadvantage of giving to the mixtures into which they enter an almost insupportable taste and smell of sulphurated hydrogen. Sulphurated ether is free from this inconvenience; it may be easily mixed with other potions, to which it gives very little smell; and as the separation of the sulphur is only occasioned by the evaporation of the ether, it may be easily prevented by keeping the mixture to which it is added closely corked. I have already adopted its use with success, administered either upon sugar, or with any appropriate vehicle: several physicians of my acquaintance, for whom I have prepared it, have likewise employed it with advantage: and I hope, ere long, to be able to flatter myself as having added an efficacious medicament to the art of healing. Sulphurated ether is a good medicine.

The sulphurated ether may be also successfully employed to detect the adulteration of wine with preparations of lead: in addition to the facility with which this ether precipitates the lead, in the form of a black sulphur, it possesses the advantage of introducing nothing into the wine that can deceive as to its quality, which sometimes happens even to those who are accustomed to use the solution of sulphur of potash. It may be used as a test for lead in wine.

I am now occupied in the crystallization of sulphur dissolved in ether, the result of which I shall lose no time in laying before the public.

On

XVI.

On the Utility of scientific periodical Publications. In a Letter from Mr. Richard Winter. *To which are added, some Experiments of Heat produced by a Blast of Air from Bellows.*

To Mr. NICHOLSON.

Dear Sir,

Periodical works are of modern invention.

THE advantages derived from scientific periodic publications, are an acquisition which former philosophers were not possessed of; and it was not until the last century they were first instituted. The rapid progress of science and information since that period, would be a sufficient argument in favour of their decided utility, without any reference to systematic treatises published, of undoubted merit, and sanctioned by universal approbation.

Advantages derived from scientific Journals.

To the active and ingenious mind in early life this mode of information is invaluable. Besides furnishing new ideas to the young student; they point out the precise state of the different branches of human knowledge; they teach him the necessary caution for conducting experiments with vigour and accuracy, instead of drawing conclusions from a few insulated analyses, or imagining that his data are sufficiently perfect for establishing new systems. By reading these publications it is that he will enlarge his general conceptions, and will learn to emulate the various illustrious characters of all the enlightened countries of the world. In these treatises his views will not be confined to one object, but he will contemplate a scene continually varying. The physiology and phenomena of the animal and vegetable kingdoms; the actions and re-actions of the different elementary substances in nature, and their combinations with each other, will pass in succession under his observation.

The great physical laws which constitute and maintain the equilibrium of the world, are inserted in respectable works of this nature as they are discovered and demonstrated, while the errors of former philosophers are detected and exposed; by which means he has an opportunity of ascertaining the value and correctness of those works he may be already in possession of.

To

To thofe who confult an Encyclopædia for fcientific matter, thefe publications are of indifpenfable utility, by continually pointing out the numerous improvements as they become public, and by that means the general fyftem of philofophical knowledge is kept to the level of the exifting ftate of difcovery.

To the mechanic a repofitory of this kind muft be highly ufeful, as the receptacle in which he may record his labours and improvements, and fecure to himfelf the well-earned fame of his difcoveries, at the fame time that he derives advantage from others following his example in their contributions to the general fund of fcience.

In fhort, there is no clafs of individuals but may profit from this method of extending ufeful knowledge. The fmall fum of feven-pence or eight-pence a week to any economical perfon is trifling, and there is no doubt but every enquirer will find fomething of which he may abridge himfelf, in order to become poffeffed of fuch an affemblage of facts and opinions. He is as it were making himfelf intimate with a clafs of men whofe names will be read with admiration by a grateful pofterity. It is only by familiarizing the mind with the fublime objects of fcience, and diffufing them over the face of the earth, that we can expect to eftablifh that fpirit of philanthropy and focial order, which is fo neceffary to the happinefs of the human race.

I will leave it to your judgment to abridge, or cancel the whole of this paper, as it would perhaps exclude more valuable fubjects.

I am, Sir,

With the greateft refpect,

Your very humble fervant,

RICHARD WINTER.

21, *Bolfover Street*,
Dec. 14, 1805.

The following experiments were made in order to afcertain whether a current of air projected upon a thermometer would increafe or diminifh the temperature. I made ufe of a pair of common bellows, the contents of which, when opened, were 95 cubic inches; the diameter of the end of the pipe was

The thermometer raifed by a blaft from bellows.

was $\frac{1}{10}$ths of an inch. The thermometer was adapted to Fahrenheit's ſcale, and the reſults of three experiments are exhibited in the following table:

		Number of Blaſts.	Time of blowing.	Therm. roſe.
Exp. 1.	-	425	6 minutes.	4°
2.	-	222	3 ——	3.75
3.	-	217	3 ——	3.7

The current of air was directed againſt the bulb of the thermometer. The diſtance of the pipe out of which the air iſſued, was half an inch from the bulb. The experiments were repeated with every caution poſſible for twelve times, and always with the ſame reſults.

Mr. Dalton obſerved (Philoſ. Journal, III. 160), that the thermometer fell on exhauſting the veſſel in which it was placed, and roſe again on re-admitting the air. It is probable that the riſing of the thermometer in my experiments may be referred to the ſame cauſe, *viz.* the greater capacity of a vacuum for caloric than atmoſpheric air.

XVII.

An Account of two interſecting Rainbows, ſeen at Dunglaſs in Eaſt Lothian in July laſt, was communicated by Profeſſor PLAYFAIR *.

Large rainbow where the ſun was 2° high.

"AT Dunglaſs, where I happened to be in the beginning of July laſt, 1799, our attention was called one evening, a little before ſunſet, to a very large and beautiful rainbow, formed on a cloud which hung over the ſea, and from which a ſhower was falling at a conſiderable diſtance to the S. E. The ſun was about 2° high, ſo that the arch was not much leſs than a ſemicircle, with its higheſt point elevated about 40°. At the point where the northern extremity of this arch touched the horizon, another arch ſeemed alſo to ſpring from the ſea, diverging from the former at an angle of 3° or 4°, on the ſide toward the ſun.

Another interſecting bow, over the ſea.

* Edin. Tranſ. 1805. Hiſtory, p. 8.

This

This arch did not exceed 7° or 8° in length; it was of the same breadth with the principal bow; it had the colours in the same order, and nearly of the same brightness; or if any difference was discernible, it was, that the transition from one colour to another was not made with so much delicacy in the last-mentioned rainbow as in the former. It was a short portion,

We recollected that a phenomenon similar to this is described in the *Philosophical Transactions*, as having been seen at Spithead, and that it is ascribed by the gentleman who observed it to the reflection of the sun's rays from the surface of the sea, so as to fall on the cloud where the rainbow was formed. This hypothesis seemed to agree exactly with the phenomenon now before us. and was apparently formed by reflection of the sun's rays

The accidental rainbow, for so it may be called, was seen only at the extremity where the principal arch rose from the sea, and where of consequence, the sun's rays, reflected from the surface of the water, at that moment very smooth, might fall on the drops of rain. The other parts of the cloud could not receive rays so reflected, as the land intervened, and there, accordingly, no vestige of the accidental rainbow was observed. from the smooth water.

The accidental rainbow lay, as was already said, on the side toward the sun, and this is agreeable to the hypothesis; for the rays that after reflection from the surface of the water fell on the drops of rain, must have come as from a point as much depressed below the horizon, as the sun was at that instant elevated above it. The axis of the accidental rainbow must therefore have made with the axis of the principal, an angle equal to twice the sun's elevation, and its center must have been elevated by that same quantity above the centre of the other, so that if it had been complete, it would have been wholly between the principal rainbow and the sun. Its center was above the horizon;

The only cirumstance in which the appearances did not perfectly correspond with this hypothesis, was, that the two rainbows did not intersect one another in the horizon, but rather a little above it. This however, ought to have no great weight, as the reflected image of the sun cannot have presented to the cloud a disk so regular and well defined as the sun itself and the but the intersection was not quite as low as the horizon.

the accidental rainbow muſt have ſomewhat participated of this indiſtinctneſs *.

The inclination of the two arcs computed,

When phenomena of this kind occur, it would afford a ſure means of trying the juſtneſs of the explanation, if the inclination of the two bows were obſerved, and alſo the ſun's altitude at the ſame time. Theſe two things are neceſſarily connected; for if we call I the angle of their interſection, E the elevation of the ſun, and S the angle ſubtended at the eye by the ſemidiameter of the rainbow, if complete, an angle which is conſtantly the ſame, and nearly equal to 42°, it is eaſy to infer from ſpherical trigonometry, that $\sin \frac{1}{2} I = \frac{\sin E}{\sin S}$.

and was a little more than the eſtimate.

Computing from this formula, the inclination of the two bows in the preſent inſtance comes out nearly 5°; ſomewhat greater than I was inclined to eſtimate it by the eye.

Phenomena of this kind can but rarely occur, as the neceſſary conditions will not often come together. The principal rainbow muſt be over the ſea; the ſea itſelf muſt extend ſomewhat on the ſide toward the ſun; it muſt be ſmooth and tranquil, and the ſun ſo low that the light reflected from the water may be conſiderable. Were it ever to happen that the accidental bow was completely formed; the effect could not fail to be very ſtriking.

* As the place of interſection will lie in a plane paſſing through the eye of the obſerver and parallel to the plane of reflection; does not this fact afford ground for a ſuſpicion that the reflection, at this low altitude, was made, not from the ſurface of the ſea, but from that of the ſtratum of vapour which occaſions looming, and has been ſo well treated of by Dr. Wollaſton and others, (ſee our Journal, VI. 46, and elſewhere), and that this ſtratum was higher farther out at ſea than near the coaſt?—N.

Notice

XVIII.

Notice of a Collection of Memoirs which have lately appeared at Paris, being Part of a Work on which the celebrated Lavoisier was employed till the lamented Close of his Life; with a Translation of that Memoir, in which he claims the modern Theory of Chemistry as his own exclusive Discovery. Received from Mr. W. A. Cadell, at Paris.

To Mr. NICHOLSON,

SIR, Paris, Oct. 27, 1805.

I HAVE translated the two following passages (pages 4 and 5) from a work which has lately appeared in two volumes octavo, entitled *Memoires de Chimie*. They will prove interesting to the readers of your Journal. The first is the notice prefixed to the work by Madame Lavoisier, (now countess of Rumford) it is written with the eloquence of real feeling, and I refer to it for an account of the nature of the work; the second proves completely that the new theory of chemistry is due to M. Lavosier alone. I also send you the titles of the papers of which the work is composed. I am, See introductory letter.

Your very humble servant,

W. A. CADELL.

CONTENTS OF THE TWO VOLUMES.

PART. I.—*General Views on Caloric: its Effects; the Manner of measuring it, and the Formation of Liquids and Fluids.*

1st mem. on caloric, by *Lavoisier*. Mem. Ac. des Sci. 1777. Contents of the memoirs arranged by Lavoisier.

2. On caloric, and the means of measuring its effects. *ib.* 1780. *Lavois. et Laplace.*

3. Supplement to the preceding. *Lavois. and Laplace.*

4. On some of the principal phenomena of chemistry.—*Seguin.* Soc. Philom. 1790.

5. On the natural zero. *Seguin.* Annal. de Chim. 1790.

6. On the effects of heat in dilating metals and glass, &c. *Laplace and Lavoisier.*

7. On the passage of solids to a state of liquidity by means of heat. *Lavoisier.*

8. On the action of heat on liquids from their freezing point to that of their vaporization. *Lavoisier.*

On

Contents of the memoirs arranged by Lavoisier.

9. On the combination of heat with different evaporable substances and the formation of several fluids. *Lavoisier*. Mem. Ac. Sci. 1777.

10. On the electricity that is absorbed by bodies that pass to the state of vapour. *Lavoisier and Laplace*, M. A. S. 1781.

11. On the action of heat on some aerial fluids from the freezing to the boiling point. *Guyton and Duvernois.*

12. On some substances which are constantly in the state of aerial fluid at the ordinary temperature and pressure of the atmosphere. *Lavoisier.*

13. Memoir on some liqnids which can be obtained in an aerial form at a degree of heat a little above the mean temperature of the earth. M. A. S. 1777.

14. General views concerning the formation and constitution of the atmosphere.

15. On the cause of some of the principal phenomena of meteorology.

PART II. *On the Decomposition of atmospheric Air, its Analysis and the Conversion of its Principles into the solid or liquid State.*

SECTION I. *On the Decomposition of Air by metalic Substances and the Formation of Oxids.*

1. Memoir on the action of mercury upon atmospheric air. *Lavoisier.* In pars. in M. A. S. 1777, p. 186.

2. On the decomposition of atmospheric air by the oxidation of lead and tin performed by means of a burning glass under a glass receiver. *Lavoisier.* Opusc. Chim. chap. 6. pub. in 1773.

3. On the oxidation of tin in close vessels, &c. *Lavoisier.* Read in 1774.

4. On the decomposition of atmospheric air by iron. *Lavois.*

5. Historical details on the cause of the augmentation of weight that metals acquire when heated with contact of air. *Lavoisier.* It is the paper of which I send you the translation.

SECITON II. *on the Decomposition of Air by simple inflammable Substances which form Acids by their Combustion.*

1. Memoir on the decomposition of air by phosphorus, and the formation of phosphoric acid. *Lavoisier.* Opusc. Chim.

2. Sup-

2. Supplement to the preceding paper. *Lavosier* M. A. S. 1777. Contents of the memoirs arranged by Lavoisier.

3. Proving that caloric disengaged from vital air during combustion is not possessed of weight susceptible of being estimated.

Process usually employed for obtaining phosphorus, phosporic acid, and phosphoreous acid. *Seguin.*

5. Memoir on the combustion of phosphorus employed as an eudiometer. *Seguin.*

9. On the decomposition of air by sulphur, the formation of sulphureous and sulphuric acid, and the use of sulphurets in eudiometry. *Lavoisier.*

7. On the process employed in commerce to obtain the sulphureous and sulphuric acids. *Seguin.*

8. On the decomposition of air by charcoal and the formation of carbonic acid. *Lavoisier.* M. A. S. 1781.

2. On the formation of nitric acid by the immediate combination of azotic gas and vital air. *Seguin.*

10. On the eudiometer composed of nitrous gas. *Seguin.*

SECTION III. *On the Decomposition of Air by those simple inflammable Substances which do not form Acids by their Combustion.*

1. Mem. Account of the last experiments on the decomposition and recomposition of water. *Lavoisier* Journal Polytip. February, 1786.

2. Shewing that water is not a simple substance, but a binary combination of hydrogen and oxigen. *Lavoisier.* read in 1783.

3. Shewing by the decomposition of water that it is not a simple substance, and that there are several means of obtaining in abundance, the hydrogen gas, which is one of its elements. *Lavosier* M. A. S. 1781.

4. Report on the paper of Seguin, which treats of the combustion of hydrogen gas with vital air. *Lavoisier, Laplace, &c.*

5. On the combustion of hydrogen gas in close vessels. *Fourcroy, Vauquelin,* and *Seguin,* read in 1790.

PART IV. *On the principal Phenomena of the Animal Œconomy.*

1. Mem. Experiments on the respiration of animals, and the change which takes place in air in the lungs. M. A. S. 1777, p. 185.

2. The

2. The alterations that the air undergoes during respiration. *Lavoisier*, read in 1785.

3. Memoir, report on a paper of Seguin's concerning respiration and animal heat. *Maquer and Fourcroy.*

4. On respiration and animal heat. *Seguin.*

5. On the respiration of animals. *Seguin and Lavoisier*, read in 1790.

Notice prefixed to the Work (by (Mad. Lavoisier) countess of Rumford.

Intention of Lavoisier to republish his memoirs,

In the year 1792 M. Lavosier had formed the design of making a collection of all his memoirs which had been read at the academy during the twenty years preceding. This would have formed in some degree the history of modern chemistry.

In order to render this history more interesting, and more complete he had proposed to insert the memoirs of those, who having adopted his theory, had published experiments in support of it.

in eight volumes.

This collection was to have been comprised in about eight volumes.

All Europe is acquainted with the cause which prevented their completion.

Parts recovered.

The portion that have been recovered are, the first volume almost entire, the whole of the second, and some sheets of the fourth.

Several men of science expressed a desire for their publication: this was received with hesitation—it is difficult not to be under apprehensions when we are intrusted with the power of publishing the unfinished work of a man justly celebrated. When we have lost the object of our affections and veneration, we should employ an impartial criticism, in order to offer to the public those of his works only which may augment his fame.

Madame Lavoisier has printed them.

We should have persisted, and these fragments would not have appeared, had they not contained a memoir of M. Lavoisier (inserted below page 5) in which he reclaims the modern theory of chemistry as belonging to himself, and states the facts in support of his claim.

It is consequently a duty towards him to fix the opinion of men of science concerning this point.

Lavoisier was employed on this

Their indulgence is requested for the errors that may exist in some other parts of the collection. It will be granted when

 they

they are informed that the greateſt part of the proof ſheets were reviſed in the laſt days of the author's life; and that whilſt he knew that his aſſaſſins were premeditating his death, M. Lavoſier, calm and intrepid, employed his laſt moments in a work which he conſidered as uſeful to ſcience, and gave a great example of that ſerenity which a virtuous and enlightened man can preſerve in the midſt of the moſt ſevere calamities.

work in the laſt moments of his life.

PART II.—SECT. I. *Fifth Memoir*, (Tom. II. p. 78.)

Hiſtorical Details concerning the Augmentation of Weight which the Metals acquire when heated with Contact of Air. (By Lavoiſier.)

IT is not my object in this paper to give a compleat hiſtory of the different opinions that have been ſucceſſively adopted by the chemiſts and natural philoſophers on the cauſe of the augmentation of weight in metals expoſed to the action of heat; ſuch a hiſtory would only ſerve to ſhew how much the minds of men are ſuſceptible of being led aſtray when they give themſelves up to the ſpirit of theory, and how eaſily we are deceived by reaſoning, if it is not perpetually rectified by experiment. John Rey, a phyſician *(medecin)* little known is one of the firſt authors who has written on this ſubject; he lived in the beginning of the 17th century at Bugue in Perigord, and kept a correſpondence with the ſmall number of perſons who cultivated the ſciences at that time.

Limit of this hiſtorical memoir.

John Rey an early writer on combuſtion.

Neither Deſcartes nor Paſcal had yet appeared; the vacuum of Boyle, and that of Toricelli, the cauſe of the aſcent of liquids in tubes void of air, were unknown; experimental philoſophy did not exiſt; a profound darkneſs reigned in chemiſtry. Nevertheleſs, J. Rey, in a work publiſhed in 1630, with a view of determining the cauſe of the augmentation of weight which takes place in lead and tin during their oxidation, diſplayed views ſo profound, ſo analogous to the facts which have been ſince confirmed by experiment, ſo conformable to the doctrines of ſaturation and affinity, that for a long time I could not help ſuſpecting that the eſſays of J. Rey had been compoſed at a much later period than that announced on the title page of the book.

His philoſophy far exceeded that of his cotemporaries.

J. Rey, after having refuted ſucceſsfully, not by facts (for at that time the art of making experiments was in its infancy) but by concluſive reaſoning, the different cauſes to which the aug-

He contends that metals gain weight from the air in oxidation.

augmentation of weight of metallic oxides might be attributed, expresses himself as follows in his 16th essay: "to this question then, supported on the grounds already mentioned, I answer and maintain with confidence, that the increase of weight arises from the air of the vessel, which is condensed, rendered heavy, and adhesive, by the violent and long continued heat of the furnace; this air mixes itself with the calx (frequent agitation conducing) and attaches itself to the minutest molecules, in the same manner as water renders heavy sand which is agitated with it, and moistens and adheres to the smallest grains.

He opposes other current opinions. J. Rey combats in this work the opinion of Cardan (*lib. 5. de subtilitate*) on the augmentation of weight of metallic oxides; that of Scaliger, that of Cœsalpinus, who ascribed this augmentation to a soot condensed and reflected by the furnace, which soot, according to their opinion, fell down upon the metal. He shews likewise that the augmentation of weight proceeds neither from the vessel, nor from any emanation of the charcoal, nor from the humidity of the air. It is difficult to conceive how J. Rey could attain to these conclusions by the force of reasoning alone, without experiment, and ignorant as he was of many of the preliminary date.

His doctrines were not received by Boyle, It appears that towards the end of the last century, when Boyle and some of his cotemporaries created the new science of natural philosophy, of which the ancients had not the slightest notion, the work of J. Rey was entirely forgotten.—Boyle, in his treatise on the weight of flame and of fire, published in 1670, that is 40 years after the publication of Rey's work, makes no mention of it; proceeding upon some illusory experiments, he still maintained at that time that the augmentation of weight which the metals acquire by their oxidation arises from the fixation of fire.

—nor by Lemery. Lemery, who was an exact and scrupulous observer, embraced the same opinion: he attributes the oxidation of metals and their augmentation in weight which accompanies that operation, to the combination of igneous particles with the metal.

Opinion of Charras, Charras, cotemporary of Lemery, ascribed that augmentation to the acids of the wood and charcoal, which as he supposed penetrated the vessels and entered into combination with the metals. Since that time the same acid of wood and ch-

coal has re-appeared under the name of *acidum pingue*, igneous acid, and under several other denominations which it would be superfluous to enumerate.

Staahl could not be ignorant of the fact that metals exposed to heat acquire an increase of weight; and yet he not only did not attempt to explain it, but also the system under which he classed the whole of the chemical pheuomena, and which after him has been so much extended, is absolutely in contradiction with this capital fact. —and of Staahl.

Staahl supposed that the metals are composed of a metallic earth, and an inflammable principle, which he named phlogiston; he pretended that they lost this principle by their oxidation, and that they could not return to the metallic state unless the phlogiston they had lost was restored to them.

It was difficult to imagine how the metals acquired weight, whilst, according to Staahl's doctrine, they lost a part of their substance, and on the other hand, how they diminished in weight, when they recovered one of the principles which they had lost; it was one of the chief difficulties that could be proposed against the theory of Staahl, this difficulty however, has not hindered the theory from having a success of limited duration. Difficulties of the phlogistic system.

Guyton Morveau has made some unsuccessful efforts to palliate this contradiction, in his dissertation on this subject, under the title of *Degressions Academiques*; he supposes that phlogiston is lighter than atmospheric air; and he concludes that all bodies that acquire phlogiston should lose a part of their weight; that, on the contrary, those which lose phlogiston should augment in weight. This explanation would have been tenable, had the augmentation of weight acquired by the metallic oxides been equal only to the weight of the air displaced; or, which is the same thing, if it had disappeared on weighing in vacuo; but the augmentation is much too considerable to admit of being attributed to that cause, since in some metals it exceeds one third of their weight. It is necessary then, either to give up the explanation of Guyton Morveau, or to suppose that phlogiston has a negative gravity, a tendency to recede from the centre of the earth, a supposition incompatible with all the facts admitted by the disciples of Staahl. Morveau's endeavours to remove them.

Such was the state of the science when a set of experiments undertaken in 1772, upon the different kinds of air or gas History of the authors experiments.

which

which are difengaged in effervefcence,' and a great number of other chemical operations, difcovered to me demonftratively the caufe of the augmentation of weight that the metals acquire when expofed to heat. At that time I was not acquainted with J. Rey's work upon the fubject, publifhed in 1630; and had I known it, I fhould have confidered his opinion in the light of a vague conjecture, which did honour to the genius of the author, but required the attention of chemifts in order to afcertain the truth of the opinion by experiment. I was young, I had newly entered the lifts of fcience, I was defirous of fame, and I thought it neceffary to take fome fteps to fecure to myfelf the property of my difcovery. At that time there exifted an habitual correfpondence between the men of fcience of France and thofe of England; there was a kind of rivality between the two nations, which gave importance to new experiments, and which fometimes was the caufe that the writers of the one or the other of the nations difputed the difcovery with the real author; confequently I thought it proper to depofit on the 1ft of November 1772, the following note in the hands of the fecretary of the Academy. This note was opened at the meeting of the 5th of May following, and mention of thefe circumftances marked at the top of the note. It was in the following terms:

He finds that fulphur and phofphorus gain weight by combuftion, and that the gain is from air abforbed. He infers that the phenomenon is general and difengages air from litharge on reducing it in clofed veffels.

"About eight days ago I difcovered that fulphur in burning, far from lofing, augments in weight; that is to fay, that from one pound of fulphur much more than one pound of vitriolic acid is obtained, without reckoning the humidity of the air; phofphorus prefents the fame phenomenon; this augmentation of weight arifes from a great quantity of air, which becomes fixed during the combuftion, and which combines with the vapours."

"This difcovery, which I confirmed by experiments which I regard as decifive, led me to think that what is obferved in the combuftion of fulphur and phofphorus might likewife take place with refpect to all the bodies which augment in weight by combuftion and by calcination, and I was perfuaded that the augmentation of weight in the calces of metals proceeded from the fame caufe. The experiment fully confirmed my conjectures; I operated the reduction of litharge in clofe veffels with Hales's apparatus, and I obferved that at the moment of the paffage of the calx into the metallic ftate, there was a difengage-

disengagement of air in considerable quantity, and that this air formed a volume at least 1000 times greater than that of the litharge employed. As this discovery appears to me one of the most interesting which has been made since Staahl, I thought it expedient to secure to myself the property, by depositing the present note in the hands of the secretary of the academy, to remain secret till the period when I shall publish my experiments."

(Signed) LAVOISIER.

Paris, 1st November, 1772.

Comparing this first note with that which I had deposited at the academy the 20th of October preceding on the combustion of phosphorus, with the paper which I read at the academy of the public meeting of Easter, 1773, and lastly with those that I have published successively, it is easy to perceive that I had conceived so early as the year 1772 the general idea of the theory of combustion which I have since published. Whence he vindicates his right to the modern theory of combustion in 1772.

This theory which I have considerably developed in 1777, and which almost at that period I brought to the degree of perfection in which it is at present, was not begun to be taught by Fourcroy till the winter 1786-1787; it was not adopted by Guyton Morveau till a later period; and Berthollet wrote still in the language of the phlogistic doctrine in 1785. This theory then is not, as I hear it called, the theory of the French chemists; it is *mine*, and it is a property which I reclaim before the tribunal of my cotemporaries and of posterity. Others undoubtedly have contributed to its perfection, but I hope that no one will dispute with me, all the theory of oxidation and combustion; the analysis and decomposition of air by metals and inflammable bodies; the theory of acidification; more accurate knowledge on the nature of a great many acids, and particularly the vegetable acids; the first notions on the composition of vegetable and animal substances; the theory of respiration, in which Seguin co-operated with me; the present collection will present all the papers on which I found my claims; the reader will judge. —which was not adopted by other chemists till many years afterwards. The claim specifically stated.

ANNOTATION.—W. N.

It was my intention to have pointed out how far the earlier chemists, as well as some of the contemporaries of this deservedly Notice of the early inventors

of the theory of combustion.

edly celebrated philosopher, are intitled to rights which will greatly modify the unqualified claim he has made. I cannot now say, whether Rey did, or did not *make experiments*, but whether he did or not, he certainly must have founded his introductions upon facts; and between the observation of well established facts, and the making of direct experiments there seems to be no essential difference. How it has happened that the great Robert Hooke, who had investigated the modern theory of combustion in 1664 and published it in an ample detail on his micrographia in 1675*, and John Mayow, who soon afterwards, or about the same time established the same doctrine, and extended it to physiological results, are overlooked by our author, appears to require some discussion. I shall take an early opportunity of resuming this subject.

XIX.

On a Method of analyzing Stones containing fixed Alkali, by Means of the Boracic Acid. By Humphry Davy, Esq. F. R. S. Professor of Chemistry in the Royal Institution †.

Acid of borax very useful in analysis.

I HAVE found the boracic acid a very useful substance for bringing the constituent parts of stones containing a fixed alkali into solution.

It combines with earths by ignition and quits them to mineral acids.

Its attraction for the different simple earths is considerable at the heat of ignition, but the compounds that it forms with them are easily decomposed by the mineral acids dissolved in water, and it is on this circumstance that the method of analysis is founded.

Processes.

The processes are very simple.

Pulverize the stone and fuse with two parts boracic acid.

100 grains of the stone to be examined in very fine powder, must be fused for about half an hour, at a strong red heat, in a crucible of platina or silver, with 200 grains of boracic acid.

Digest with weak nitric acid.

An ounce and half of nitric acid, diluted with seven or eight times its quantity of water, must be digested upon the fused mass till the whole is decomposed.

Evaporate.

The fluid must be evaporated till its quantity is reduced to an ounce and half or two ounces.

* Copied in our Journal quarto series III. 479.

† Phil. Transf. Part II. for 1805.

If

If the ftone contain filex, this earth will be feparated in the procefs of folution and evaporation; and it muft be collected upon a filter, and wafhed with diftilled water till the boracic acid and all the faline matter is feparated from it. Silex if prefent will feparate.

The fluid, mixed with the water that has paffed through the filter, muft be evaporated, till it is reduced to a convenient quantity, fuch as that of half a pint; when it muft be faturated with carbonate of ammonia, and boiled with an excefs of this falt, till all the materials that it contains, capable of being precipitated, have fallen to the bottom of the veffel. Precipitate the reft with carbonate of ammonia.

The folution muft then be feparated by the filter, and the earths and metallic oxides retained.

It muft be mixed with nitric acid till it taftes ftrongly four, and evaporated till the boracic acid appears free. Add nitric acid to the clear liquid.

The fluid muft be paffed through the filter, and fubjected to evaporation till it becomes dry; when, by expofure to a heat equal to 450° Farenheit, the nitrate of ammonia will be decompofed, and the nitrate of potafh or foda will remain in the veffel. Separate the boracic acid by evaporation. Decompofe the nitrate of ammonia by heat.

It will be unneceffary for me to defcribe minutely the method of obtaining the remaining earths and metallic oxides free from each other, as I have ufed the common proceffes. I have feparated the alumine by folution of potafh, the lime by fulphuric acid, the oxide of iron by fuccinate of ammonia, the mangan fe by hydrofulphuret of potafh, and the magnefia by pure foda.

XX.

Some Facts and Speculations on the luminous Phenomena of Electricity. W. N.

ABOUT eighteen years ago, I was confiderably occupied in experiments upon electricity, many of which were communicated in 1789, to the Royal Society, and were publifhed in the tranfactions. In the twenty-third fection of that communication, fome account is given of certain changes which take place in the luminous appearance of metallic balls when electrified; but the phenomena were not delineated, becaufe I referved them for another opportunity. After fo long an interval Communication to the Royal Society on electricity.

terval of time, I now present them to the reader from my notes, and the sketch then made.

Three appearances of an electrified ball. It gives flashes; is then luminous, and then gives flashes of another kind.

Sept. 19, 1787. A small ball in the state of electricity called positive, threw out flashes or ramified sparks; and when the intensity was encreased, the ball itself became luminous, at the same time emitting the flashes. When the electricity was still more strongly excited the flashes ceased, and a circle of light, extending about 45 degrees round the point farthest from the stem, was seen on the ball, and a strong wind proceeded from it.

Experiment with a ball $1\frac{1}{2}$ inch diameter.

A ball of one inch and a half diameter was used; and electricity communicated by means of a cylinder nine inches diameter, having its cushion eight inches long. The excitation was strong enough, by slow turning with a single winch, to throw out large brushes of light. When the rotation was quicker, the flashes disappeared, and the circle of light was seen, having a bright speck moving irregularly round in its periphery. Quicker turning threw out brushes of light very different from the others: These were less luminous in the branches; many started out at once with a hoarse sound. They were greenish at the point or surface of the ball, reddish in the stem, and ramified sooner. Half a dozen were sometimes seen flashing out at once.

Description.

Experiment with a much smaller ball. It became luminous, and acted like a point.

A ball of four tenths of an inch in diameter was used. Moderate excitation produced a dense brush of light about two inches in length. With stronger electricity the brush disappeared, and the upper half of the ball became luminous. When the excitation was still stronger, more than half of the ball was luminous, as represented *Fig. 3, Plate I.* and sometimes a ramified flash struck out from the top. Other flashes were sometimes seen sideways when the electricity was strongest of all; but this happened seldom.

The light was faint, and seemed to be about twice the diameter of the ball. It extended more than half way down, and spread most sideways.

A large ball $2\frac{1}{2}$ inch. diam.

When a larger ball of two and a half inch diameter was used, the brushes of light flew out from three or four stems together to the length of about six or seven inches, making a hoarse noise; but they could not be made to disappear, though they seemed now and then to cease for a moment when the turning was most vigorous.

Modification of the phenomena,

The next day, when the excitation was very nearly, but not quite, as strong, it was observed that the order of these appearances could be effected by the assistance of a metallic point. Plentiful brushes were thrown out from a three inch ball, but they could not be made to disappear. When a pointed wire or a small metallic ball was presented, the effects were as follow:

by the vicinity of a point or small ball.

The point being at a great distance, the root of the brush had a luminous circle of lambent light round it on the surface of the ball. When the point was nearer, the brush disappeared, and nothing was seen but an exceedingly bright speck on the surface of the ball, which was sometimes stationary and sometimes moved about. When the point was still nearer, the speck threw out ramified sparks of the second kind, at the same time that a lambent luminous circle appeared. The speck was never in the center of the circle, but moved at a distance round the circle, irregularly, sometimes the one way and sometimes the contrary, and was sometimes stationary.

More particular description of the luminous appearances.

These two orders of brushes were entirely the same as those of the day before. The luminous brush which first appeared had a straight stem, then a broken or less luminous part, beyond which loose cotton-looking fibres flew off in radial directions, as at *Fig.* 1, *Pl. I.* The latter ramified sparks had a straight central stem, out of which well defined branches issued nearly at right angles. They much more closely resembled a tree bare of leaves.

The second brush was not larger, but rather less in its dimensions than the first.

All the phenomena visible at once.

When the ball of four-tenths of an inch was held at a certain distance from the two and a half inch ball, when electrified, the first kind of brush was seen on the side farthest from the small ball, at the same time that the second kind of spark or brush flew out towards the small ball, and the lambent luminous appearance was seen on the surface.

These are the general facts; but I have no doubt but they would present many modifications upon being repeated.

Remarks on the electric spark.

These facts may serve to assist our meditations with regard to the nature of the electric spark. In a late paper by Mr. Biot, given at page 214 of our Vol. XII. the author makes an ingenious conjecture, that the light and heat in this phenomenon may have been produced by mechanical compression

Whether Biot's theory of luminous condenſed air can be ſupported.

Warltire's fire-ball.

Combuſtion of flint and ſteel,

requires a very minute portion of metal:

Electric temperature is extremely elevated;

All metals loſe by electricity, and the ſpark paſſes only between combuſtible bodies.

Hence probably it was part of the body ſet on fire.

of the atmoſpheric air. Whether this ſuppoſition can be reconciled to the appearance of the ſpark in oil, and to ſome atmoſpheric phenomena, in which we are told of luminous balls moving apparently with little velocity through the air, and particularly that ſlowly-moving artificial fire-ball, produced once, and only once, by Warltire, as narrated in Prieſtley's work on air, may admit of queſtion. When we conſider that a particle of iron, cut off and ſet on fire in the common action of ſtriking a light, appears, from the vivacity of its combuſtion, to be a body of conſiderable magnitude, though the uſual quantity of metal would not form a ball of one thouſandth of an inch in diameter; when we conſider the prodigious elevation of temperature indicated by the exploſion of wires of all metals by the electric ſhock, particularly in thoſe beautiful and ſtriking experiments which Van Marum has publiſhed; and laſtly, when we call to mind that a metallic chain loſes part of its weight every time a ſhock is paſſed through it, and that the ſpark is never ſeen to paſs between incombuſtible bodies—conſiderable reaſons will preſent themſelves in favour of a modified ſuppoſition, that the electric ſpark may conſiſt of, or be accompanied by, a portion of the body from which it proceeds.

Fire-Balls, &c. may be electric ſparks;

Are not the atmoſpheric fire-balls or luminous meteors, the ſhooting ſtars and the ſtones which have fallen from the atmoſphere, electric ſparks upon a ſcale of immenſe magnitude?

and the ſpark a fire-ball.

If any luminous ball were to paſs with a ſwift angular motion over the field of view, it would have the appearance of a line or ſtreak of light. If it were to break in pieces many divergent ſtreaks would be ſeen. May not the electric bruſh be a phenomenon of this deſcription on a ſmall ſcale?

Facts are more wanted than conjectures.

It would not be difficult to apply this ſpeculation to the figures 1 and 2 before us; but as we are more in want of facts than of conjectures, and as it may be hoped that ſome of my readers who have the means and the time will purſue this inveſtigation, I ſhall for the preſent conclude.

SCIENTIFIC NEWS.

Anatomical Cabinet.

THERE has appeared at Berlin, a complete defcription of the anatomical cabinet of M. Walter, which the king has purchafed, almoft a year ago, for the fum of 400,000 francs. This catalogue is compofed of fixty-two printed fheets, Anatomical cabinet.

Shower of Peas,

Dr. Hiem, of Berlin, has publifhed a note, in which he explains that the peas, which were faid to have fallen from the atmofphere in a fhower at Landfchut, in Silefia, were merely tubercles which are feparated from the roots of feveral plants. Thofe in queftion, according to the Doctor, were afforded by the roots of the aquatic plant *Ranunculus Ficaria.* He pretends that an enormous mafs of thefe tubercules may have been formed in certain cavities, whence they might be carried to a diftance by the whirl or eddy of ftrong wind. He fupports his opinion by the accounts of fhowers of this nature given by the celebrated Klaproth in his Journal of Chemiftry.— The Doctor concludes by remarking, that thefe tubercles contain a farinaceous fubftance equal in goodnefs to that of potatoes, and recommends an at tention to the ficaria for this purpofe, Shower of peas.

Univerfal Language,

THE Celtic academy, in a fitting of laft April, made proof of a new difcovery by one of its members; which gives the power of correfponding, and difcourfing, with men, whofe language is unknown, with expedition, without previous ftudy, any expence, the leaft trouble, or the fmalleft labour of the mind. The proof made at that fitting by twenty-five academicians, on the languages of Europe, afcertained, that by the aid of this invention, a man may travel any where without an interpreter, demand what he wants, difcourfe on whatever fubjects can intereft any fort of travellers, and even exprefs metaphyfical thoughts. It is intended to make this difcovery public at the return of the Emperor. Univerfal language.

The above account has appeared in ſeveral publications of credit, but it is probable the account is exaggerated in ſeveral reſpects.

Turkiſh Edict in Favour of Science.

Turkiſh edict in favour of ſcience.

THE Grand Seignor has conſtituted Prince Morouſi; by a diploma written with his own hand, director general of the hoſpitals of his empire, and inſpector of the ſchools of medicine, mathematics, and *belles lettres*, which his highneſs is engaged in founding with all poſſible diſpatch. This diploma is remarkable for the great praiſes of the ſciences made in it by the Grand Seignor; as they hitherto have been in no great favour with the Mahometans. In rendering juſtice to the ſkill of the Chriſtian phyſicians, who have ſtudied at the univerſities of Halle, of Padua, and of Montpelier, the Grand Seignour remarks with much truth, that theſe phyſicians, when brought into foreign countries, often commit great errors on account of the difference of the temperature of the climates; from whence he concludes that, in order to practiſe medicine well, it is neceſſary to ſtudy in the country where the profeſſion is to be exerciſed.

Coptic Manuſcripts.

Coptic manuſcript.

THE celebrated Daniſh antiquarian, M. Zoega, is daily occupied at Rome in completing his catalogue of Cophtic manuſcripts in the Borgheſe muſeum. He intends afterwards to publiſh a new topography of ancient Rome. It is probable this work will be printed in Germany, becauſe it will require numerous engravings, which no Italian bookſeller would chooſe to go to the expence of. It is, however, not believed that M. Zoega will occupy the profeſſor's chair, which has been granted him at the Univerſity of Kiel, as he is too much accuſtomed to the fine climate of Italy to leave it willingly.

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AND
THE ARTS.

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ARTICLE I.

On the Cause of Fairy Rings. In a Letter from Mr. FLORIAN-JOLLY.

To Mr. NICHOLSON.

Assembly-house, Laytonstone, Essex,
January 13.

SIR,

SEEING by the letter of Mr. Gough inserted in the last Number of your Journal, that the cause of fairy-rings is not yet agreed upon among naturalists, I beg leave to submit to their consideration a few facts which I had occasion to remark some years ago, during a summer residence in Hampshire. The phenomenon of fairy-rings

The park of Broadlands, Lord Palmerston's seat, near Rumsey, was divided into three principal inclosures, formed by hurdles only. One of these had been lately mowed; there were cattle grazing in the next; and the other, which had afforded winter and spring fodder to some horses kept at grass, was then left to grow for an autumn crop. *This* last exhibited an immense number of fairy rings, some perfectly circular, some forming irregular curves, and others nothing but small round produced in great numbers in one division of Broadlands park and none in other parts.

round patches: In all of these the grafs grew more luxuriant and of a deeper hue: No other fungus was to be found in any of them but the efculent mufhroom. In the part lately mowed, and in that where the cattle were grazing, there was not the leaft appearance of fairy-rings.

Another field abounding with them.

In the courfe of fubfequent perambulations, I obferved in a grafs field fituated on the top of the firft high ground upon the road from Rumfey to Salifbury, appearances nearly fimilar to thofe exhibited in the growing grafs of the park. There had been all fummer, and there were ftill horfes grazing in this field: The fairy-rings were numerous, but the grafs in the rings and patches, inftead of being more luxuriant, was completely dry and blafted, and there grew two or three different fungi, all of them of thofe forts which are reckoned noxious.

They were not produced by electricity,

That the fairy-rings at Broadlands were not the effect of electricity, appears to me beyond all doubt, fince one part only of the park exhibited them, while the reft of the contiguous grounds, divided from that part by nothing more than a row of hurdles, did not fhew any fuch appearance: otherwife it muft be contended, that the electrical phenomena might take place on one fide of the hurdles and never on the other, a predilection truly fingular, and, I fhould think, difficult to be accounted for.

but by the excrement of the horfes.

Another fact which I have repeatedly obferved fince that time, has led me to fufpect that the fairy-rings, their different appearances, and the various fpecies of fungi found in them, might be produced by no more uncommon caufe than the excrements of the horfes.

Argument from the appearances in hot-beds.

The hot-beds made of horfe-dung, which I have had feveral times in my garden, have generally produced in fucceffion the fame fungi which are to be found in the different ftates of the fairy-rings. Whilft the beds are yet new, the fungi are of the fame noxious fpecies as I faw in the dry blafted fairy-rings, but when they grow cooler and more matured, efculent mufhrooms begin to grow naturally, and although no fpawn was ever put in the bed.

I have alfo remarked, that horfe-dung produced in fome feafons an immenfe quantity of mufhrooms, and hardly any in others: This might perhaps be attributed to the different quality of the hay on which the horfes had fed; and this might explain why fairy-rings are to be found in fome paftures rather than in others.

That

That fairy-rings ſhould be produced by the excrements of horſes, may be illuſtrated by a very ſimple fact, which it is in the power of every perſon to obſerve. If you let fall ſome oil upon a marble ſlab, or ſome other liquid upon ſome ſubſtance that will imbibe it, you will ſee it gradually ſpread round in a more or leſs regular form; ſometimes aſſuming the appearance of a patch, and frequently continuing to flow from the center to the circumference, where it accumulates in a much greater proportion than in the inner part of the circle, taking thus the form of a ring. Experiment with oil, to illuſtrate the deductions.

This accumulation of the fluid at the circumference may be eaſily explained. As the fluid expands, the preſſure from the center becomes gradually leſs, till at laſt there is no ſufficient force to overcome the reſiſtance oppoſed by the dry parts of the ſolid ſubſtance which has imbibed it: yet, in conſequence of the firſt impulſe, the fluid will continue to flow from the center through the ſmall channels already opened, and will thus accumulate in greater quantities at the boundaries where its expanſive motion is ſtopped.

The excrements of horſes, diluted by the rains and imbibed in the ſoil, muſt have an effect ſimilar to that juſt deſcribed. This effect muſt, beſides, greatly depend upon the nature of the ſoil and the facility with which it is pervaded by the fluid; hence the conſtant appearance of fairy-rings in ſome paſture-grounds, while none are ever to be found in others. Application.

Should you, Sir, conſider theſe remarks, and the deductions which they have ſuggeſted to me, as likely to throw ſome light upon the cauſe of fairy-rings, you are welcome to make any uſe of them you may think proper.

I am, Sir,

Your obedient humble ſervant,

J. FLORIAN-JOLLY.

II.

Experiments on the Magnetism of slender Iron Wires. By JOHN GOUGH, *Esq.*

To Mr. NICHOLSON.

SIR, *Middleshaw, January* 9, 1806.

A general maxim in magnetism stated.

THE general phenomena of magnetism have given rise to a maxim which shall be here stated in the words of a judicious writer on the subject. "The magnetism acquired by being placed within the influence or the sphere of activity of a magnet in soft iron, lasts only while the iron continues in that situation; and when removed from the vicinity of the magnet, its magnetism vanishes immediately; but with hard iron, and especially with steel, the case is quite different; for the harder the iron or steel is, the more permanent is the magnetism, which it acquires from the influence of a magnet." Cavallo on Magnetism, London, 1787, p. 30.

Remarks on this maxim.

This proposition is of great utility in the science, for it explains a variety of relations betwixt the magnet and ferruginous bodies, but I have observed one phenomenon that appears inexplicable on the principle, and consequently may be said to offer one exception to the general proposition. As my experiments on the subject are very easy, it seems adviseable to deliver the leading circumstances in the form of so many precepts because this method will assist any one desirous of pursuing the enquiry, to repeat them with ease.

An experiment consistent with the maxim.

Experiment 1. Apply either pole of a strong magnet to one end of a short horizontal bar of clean soft iron, and a particle of iron equally soft to the other end. This particle will remain suspended at the extremity of the bar until the magnet is withdrawn; but the removal of this power will dissolve the connection subsisting betwixt the two pieces of iron, and the particle will drop off immediately.

An experiment contradicting the maxim.

Exp. 2. The preceding experiment confirms the maxim stated above, when conducted according to the foregoing directions; but let it be repeated with the following alteration, and it will contradict the general proposition. In place of the particle of soft iron, substitute a piece of iron wire of number 32 in the wire drawers scale, the weight of which may amount to two or three grains. The removal of the magnet will

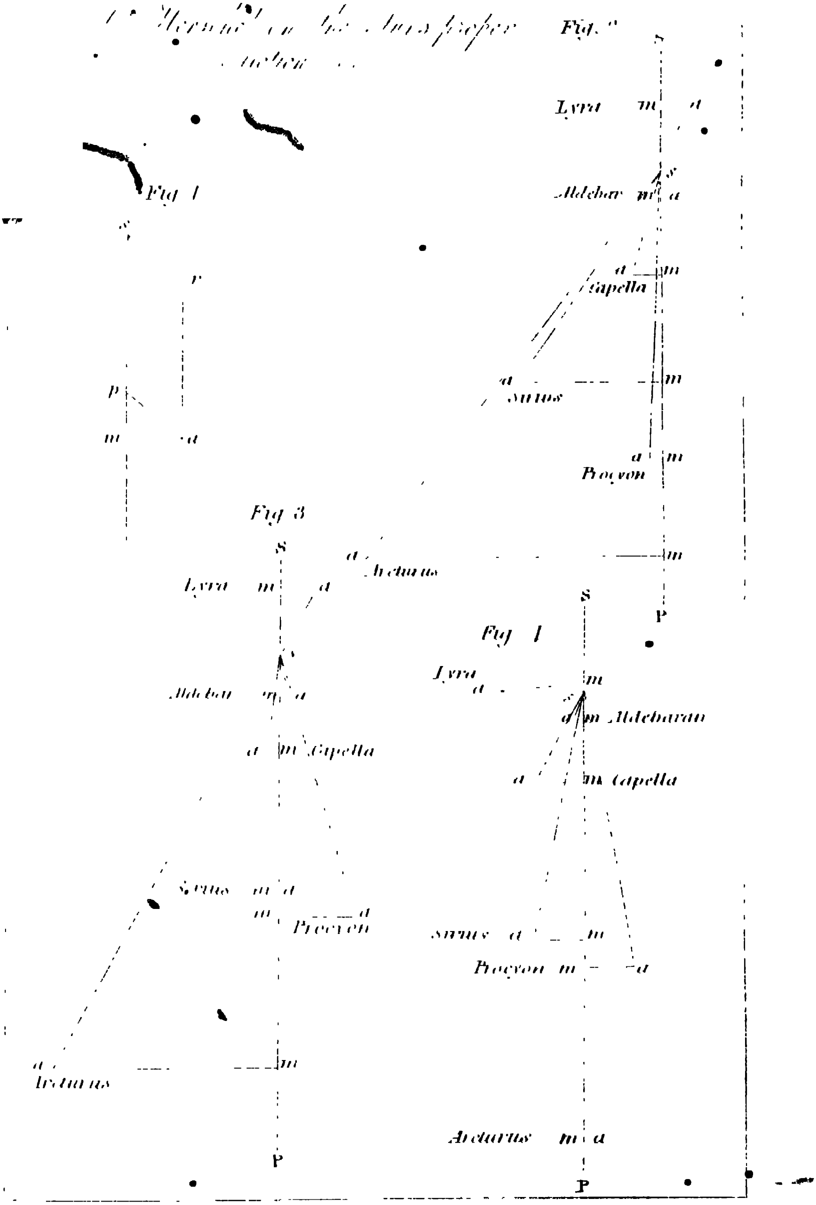


Fig. 2
Lyra m a
Aldebar m a
a m
Capella
a m
Sirius
a m
Procyon
m
S
P
Fig 1
S
P
m a
Fig 3
S
Lyra m a
a
Arcturus
Aldebar m a
a m Capella
Sirius m a
m a
Procyon
a
Arcturus
m
P
Fig 4
S
Lyra
a m
a m Aldebaran
a m Capella
Sirius a m
Procyon m a
Arcturus m a
P

will not break the connection formed by its presence between the bar of soft iron and the wire; for the latter will remain attached to the end of the former, by the extremity which was first brought into contact with the iron; if the piece of wire be removed from the end of the bar, the magnetic connexion may be revived by replacing it immediately. The same thing will happen if the wire be expeditiously transferred from the first bar to another rod of soft iron; but it loses its magnetism in the space of two or three seconds when kept at a distance from all ferruginous bodies which are capable of attracting it, and of being attracted by it. These facts prove wire of number 32 to be a magnet, the virtue of which is conditional, because its permanency depends on the presence of soft iron, and perhaps on no other circumstance; for the experiment may be repeated with success upon rusty wire of the same size, or on pieces which have been made red hot in the flame of a candle, or surrounded by sand in a crucible, in which situation they will cool much more gradually than when drawn singly through a flame.

Wires of low sizes not conditional magnets.

Exp. 3. This capacity of iron wire to preserve the magnetism imparted to it, as long as it remains in contact with a bar of the same metal, is a property confined to certain sizes; for let the first experiment be repeated with a small piece of numbers 18 or 17, not equal to half a grain in weight, and just as it comes from the hand of the workman, this piece will perform the part of a particle of soft hammered iron, that is it will drop from the end of the bar, to which it has been attached by the application of a magnet, to the opposite extremity, as soon as the magnetic influence ceases to act upon it: consequently the mere operation of drawing soft iron into wire, by forcing it through a conical hole too narrow for its present diameter, will not convert it into a conditional magnet.

The lowest size capable of conditional magnetism ascertained.

Amongst other experiments relating to the subject, I took the trouble to examine the quality of every size from 32 to 21, both inclusive; the 11 smallest wires, the extreams of which were 32 and 22, were all conditional magnets; that is, they all adhered to the bar of soft iron, to which they had been previously attached, after the removal of the magnet. Number 23 supported seven grains of lead including its own weight, without the assistance of the magnet; No. 24, $6\frac{1}{2}$ nearly; No. 32, $4\frac{1}{2}$; No. 22, no more than two grains.

As for number 21, it poſſeſſed the ſimple properties of ſoft iron: for the ſhorteſt cylinder which could be taken from a rod of this ſize by means of a cutting file, dropped from the end of the horizontal bar as ſoon as the magnet was withdrawn.

Remarks on Exp. 3d.

It is difficult to ſay, which of the 11 wires mentioned above, had the magnetic virtue in the moſt perfection, becauſe each piece differed in diameter from the reſt; beſides which, it is very well known, that a maſs of iron, of a weight and figure determinable by experiment only, is attracted by any particular magnet, more powerfully than any other maſs of the ſame metal. But the preceding trials have diſcovered one circumſtance apparently of ſome importance, for they ſhew that wire is converted into a conditional magnet by its paſſage through the 22 wordle, or wire drawers inſtrument; and that the 23d operation brings this quality in it to perfection as far as we can judge from experiment.

I here only ſpeak of wire drawn in Kendal, for I have been told, that the ſame article manufactured in ſome parts of Yorkſhire, has a much greater propenſity to become magnetical. This information was communicated to me by Mr. Morrice, a very intelligent ſuperintendant of a manufacture of cards in this town; who moreover obſerved, that wire of this deſcription acquires a degree of magnetiſm under the ſhears, which induced him, when employed in working it, to ſubſtitute a braſs gauge for the common inſtrument made of iron.

Conjectures relative to the cauſe.

The magnetic property which commences with number 22, ſeems to be common to all the finer ſizes, for I found it in the ſmalleſt wire I could procure, and which apparently did not exceed a ſtrong human hair in thickneſs.

The foregoing experiments, beſides proving that ſlender wires acquire a magnetiſm which is permanent as long as they remain in contact with iron, alſo affords an exception to a ſecond general maxim of the ſcience, which aſſerts, that the permanency of communicated magnetiſm depends on the hardneſs of the ferruginous body that receives it. This does not appear to be the caſe in experiment 2, in which wire of No. 32 did not loſe the faculty of being convertible into a conditional magnet after undergoing a red heat, a proceſs that is well known to render wire very ſoft. I even repeated the experiment with the ſame reſult on all ſizes betwixt 22 and

and 33, except 26; pieces of each sort were heated both in the flame of a candle, and in sand; all of which retained the faculty under consideration after being treated in both ways. In reality, wires that had been thus softened, seemed to be in the same condition with small nails of cast iron, considered as retainers of magnetism, though the latter are of a much harder quality; for a nail of the sort called sparrow-bills by shoe-makers exhibited the appearances described in the second experiment, after being filed down to the thickness of a small wire.

If then that kind of magnetism which I have ventured to call conditional do not depend on comparative hardness, to what cause is the phenomenon to be described? little can be offered on my part, besides probable conjecture, in answer to this question. The temperature of wire is considerably raised during its passage through the wordle; and may not we imagine with some shew of reason, that this encrease of temperature, joined to the subsequent contact of cold air, produces a new arrangement of the molecules constituting the wire which enables it to retain a portion of magnetism as long as it remains in contact with a ferruginous body? if this supposition be true, experiment proves the new arrangement to take place in the 22 wordle; when the slenderness of the wire will occasion it to cool suddenly after passing through the instrument. The reality of such changes in the texture of bodies which are not in a state of fusion, is admitted at present by experimental philosophers. I may also quote in favour of this hypothesis some valuable observations made by Gregory Watt, Esq, on the various degrees of magnetism exhibited by the same bazaltic stone under different forms of crystallization; which observations may be seen in your Journal for February, 1805.

Any attempt to explain the permanent magnetism of small wires during their connection with soft iron, and the loss of this property which ensues when the connection is broken, appears to be superfluous, because the fact is evidently analogous to the well known method of adding strength to a magnet by a gradual encrease of its load; for this operation, when judiciously conducted, gives a magnetic charge to a bar of steel already touched, which it cannot retain after the weight is removed.

I remain, &c.

JOHN GOUGH.

P. S. I neg-

P. S. I neglected to mention the following circumftance in the body of the letter. The drawing inftrument, or wordle, is made of fteel; and is it not probable that this tool, poffeffing a flight degree of magnetifm given to it by friction or otherwife, affifts in producing the neceffary arrangement, by acting upon heated and flender wires, while their molecules are in a violent motion from the preffure of the inftrument itfelf? This fuppofition has fome claim to plaufibility; becaufe a weak magnet will impart a portion of the fame virtue to a bar of tempered fteel, the particles of which are in a ftate of vibration; for a rod of this metal will acquire a degree of polarity, provided it be ftruck on the end with a hammer when its axis lies parallel to the dipping-needle.

III.

Concerning the Differences in the magnetic Needle, on Board the Inveftigator, arifing from an Alteration in the Direction of the Ship's Head. By MATTHEW FLINDERS, *Efq. Commander of his Majefty's Ship Inveftigator. From the Philofophical Tranfactions,* 1805.

The magnetic needle is affected at fea by the pofition of the fhip's head:

WHILST furveying along the fouth coaft of New Holland, in 1801 and 1802, I obferved a confiderable difference in the direction of the magnetic needle, when there was no other apparent caufe for it than that of the fhip's head being in a different direction. This occafioned much perplexity in laying down the bearings, and in allowing a proper variation upon them, and put me under the neceffity of endeavouring to find out fome method of correcting or allowing for thefe differences; for unlefs this could be done, many errors muft unavoidably get admiffion into the chart. I firft removed two guns into the hold, which had ftood near the compaffes, and afterwards fixed the furveying compafs exactly a-midfhips upon the binnacle, for at firft it was occafionally fhifted to the weather fide as the fhip went about; but neither of thefe two arrangements produced any material effect in remedying the difagreements.

fcarcely from the iron on deck.

The following table contains the obfervations for the variation of the compafs in which the differences are moft remarkable, and from which I fhall beg to point out fuch inferences as I think may be drawn from them.

TABLE,

TABLE, fhewing the Errors produced in the Magnetic Compafs by the proper Magnetifm of the Ship.

Time.	Latitude.	Longitude.	Number of compaffes ufed.	Number of fets of obfervations taken.	Place of the compafs.	Suppofed true variation.	Obferved variation.	Ship's head.	Obferver.
1801.									
Dec. AM	35° 5′ S	116° 28′ E	two	4 azimuths	binnacle	7° 0′ W	5° 59′ W	NW b. N	Commander
	Princefs Royal Harbour		three	6 —	on fhore	6 15	6 23	—	
	—	—	theodolite	1 —	—	—	6 8	—	
1802.									
Jan. 9, PM	34 1	121 20	one	2 —	binnacle	5 0	9 22	ESE	
16, AM	Goofe Ifland Bay		one	2 —	—	4 0	0 54	W	
1803.									
May 20, AM	—	—	—	2 —	—	—	6 8	E	
1802.									
Jan. 18, PM	33 37	124 10	—	2 —	—	4 30	5 44	NNE	
20, PM	32 33	125 35	—	2 —	—	—	7 15	E b. N	
21, PM	32 30	125 48	—	2 —	—	—	4 45	S	
22, PM	32 30	126 7	—	2 —	—	4 15	6 13	NE b. E	
23, PM	32 21	126 33	—	1 amplitude	—	4 0	4 18	S b. E	
24, AM	32 5	128 15	—	2 azimuths	—	3 0	6 4	E b. N	
26, PM	32 15	128 15	three	6 —	—	—	3 7	S b. E	Lt. Flinders
30, AM	32 18	132 29	one	2 —	—	0 30	1 41	SSE	Commander
Feb. 4, AM	No. 4, bay in ifland e		—	2 —	—	0 15	2 23	Eafterly	
5, AM	32 39	133 55	—	1 amplitude	—	—	1 56	E b. S	
6, AM	32 36	133 58	three	6 azimuths	—	—	1 0 E	NW	Lt. Flinders
26, PM	34 3	135 20	one	2 —	—	1 5 E	1 33 W	SE b. E	Commander
—, PM	34 5	135 24	—	1 amplitude	—	1 5	3 56 E	SW	

TABLE

TABLE, shewing the Errors produced in the Magnetic Compass by the proper Magnetism of the Ship.

Time.	Latitude.	Longitude.	Number of compasses used.	Number of sets of observations taken.	Place of the compass.	Supposed true variation.	Observed variation.	Ship's head.	Observer.
1802.									
Feb. 18, AM	34° 50′ S	135° 32′ E	three	6 azimuths	binnacle	1° 12′ E	1° 12′ E	S	Lt. Flinders
Mar. 1, PM	In No. 10, bay		theodolite	1 —	on shore	1 39	1 39	—	Commander
5, PM	—	—	one	2 —	binnacle	1 39	0 53	S b. E	
17, PM	34 12	137 20	—	1 amplitude	—	2 15	4 38	SW b. S	Commander
18, PM	34 23	137 36	one	2 azimuths	binnacle	2 15	0 35	SE	
21, AM	35 33	137 15	three	6 —	—	2 40 E	1 10	SE b. S	
23, AM	Kanguroo Island		two	4 —	—	2 58	6 31	SSW	
26, AM	35 10	137 41	one	1 amplitude	—	2 45	1 49	NE b. N	
27, AM	35 21	137 52	one	1 —	—	2 50	1 49	SSE	
April 6, AM	Kanguroo Island		—	2 azimuths	on shore	2 58	2 58	—	
10, AM	35 47	139 15	—	1 amplitude	binnacle	3 0	5 11	W b. S	
11, PM	35 53	139 26	—	2 azimuths	—	3 0	0 50	SE	
13, PM	36 45	140 5	—	2 —	—	3 30	1 25	SE b. S	
16, AM	37 55	139 55	—	2 — 1 amplitude	—	4 15	2 20	—	
17, PM	37 57	139 56	—	2 azimuths	—	4 15	2 2	NE	
22, AM	39 38	144 50	—	2 —	—	7 45	11 52	WSW	
26, PM	38 35	144 25	—	2 —	—	7 30	3 41	NE b. E	
—, AM	38 38	144 35	—	1 amplitude	—	7 30	6 48	NE b. N	

Note. All the compasses made use of on board the Investigator were of Walker's construction, one excepted, which was made by Adams, and used only on July 22, 1801.

It

It is apparent that ſome of the obſerved variations in the above table are 4°, leſs and others 4° greater than the truth; and it may be remarked that when this error is weſtward, the ſhip's head was eaſt or nearly ſo, and when it was eaſtward the head was in the oppoſite direction. When the obſervations agree neareſt with what was taken on ſhore, or with what may be deemed the true variations the ſhip's head was nearly north or ſouth; and a minute inſpection of the table will favour the opinion, that the exceſs or diminution of the variation was generally in proportion as the ſhip's head inclined on either ſide from the magnetic meridian.

The errors were about 4° each way, and the north end of the needle deviated as if repelled by the ſhip's head, &c.

After I had well aſcertained the certainty of a difference in the compaſſes, ariſing from an alteration in the point ſteered, I judged it neceſſary, when I wanted a ſet of bearings from a point where we tacked the ſhip, to take one ſet juſt before and another immediately after that operation: ſome ſpecimens of theſe here follow.

Other obſervations.

1802.		*Head* ESE.		*Head* SW b. W.
April 13th, 11h 32′ AM	Le Geographe Rocks	N 55° to 71° E		
	Ξ point	N 4 W	after tacking	N 9° W
—	Π point	S 32 E	-	S 40 E.
		Head SE b. E.		*Head* W.
April 14th, 9h 29′ AM	Π point rocky, inner part	N 39° E.	after tacking	N 30° E
	——— projecting part	N 67 E	-	N 59 E
—	Furtheſt viſible extreme from deck	S 51 E	-	S 55 E.
		Head ENE.		*Head* SW b. S.
April 15th, 11h 50′ AM	N, the weſtern part	N 15° W.	after tacking	N 21° W
	A peaked hummock	N 19 E	-	N 15 E
—	Furtheſt extreme from deck	S 53 E	-	S 61 E
—	Centre of a naked ſandy patch	E	-	E 5 N.

Variation per amplitude April 15, AM, taken with the ſurveying compaſs } 4° 8′ E, ſhip's head being S.

April

		Head E.	*Head* SW b. S.
April 15th, 5h PM	The peaked hummock	N 12° W	after tacking N 18° W
	Former extreme, a projection	S 59 E	S 64 E
—	Naked ſandy patch, diſtant 3½	N 33 E	N 31 E.

Limits of error in obſerving bearings on ſhip-board.

From ſome little change of place after tacking the ſhip, and from the part whoſe bearing was ſet not being perhaps the individual ſpot in both inſtances, the difference between the ſeparate bearings in any ſet will not be always the ſame: to theſe cauſes for error alſo may be added inacurracies in taking the angles ariſing from the motion of the ſhip and compaſs, from the view of the object being obſtructed by the rigging, maſts, or ſhip's upper works, and from too much haſte to get the bearings before the ſhip's place was materially altered. Even in the Table of azimuths and amplitudes greater accuracy than one degree muſt not be looked for; and in ſhip-bearings two or even three degrees is not, I believe, too great an allowance for error, unleſs in very favourable circumſtances.

They may amount to two or even three degrees.

Reſults ſimilar to thoſe firſt ſtated.

Without attending to ſmall differences, it is evident that the bearings correſpond with the obſervation in requiring a leſs eaſt variation to be applied when the ſhip's head was eaſterly, and a greater when it was to the weſtward, in order to get at the true direction of the object *. When examining the north and

* As a ſpecimen of the plan I followed in protracting ſuch bearings as the above, take the ſet of April 15, A M, when the true variation appears to have been 4° E. On the firſt bearing the ſhip's head was ſix points on one ſide of the meridian, and on the ſecond it was three points on the other ſide, the mean is one point and an half on the eaſt ſide; now for this one point and an half I allow 1° of error, which, as it is on the eaſt ſide of the meridian, and the variation is eaſterly, muſt be ſubtracted: the variation then to be allowed upon the mean between the bearings before and after tacking will be 3° E, from which the true bearings will ſtand as follows:

April

and eaſt coaſts of New Holland, I always endeavoured to take the angles on ſhore with a Troughton's portable theodolite, and to obſerve for the variation in the ſame places, that all the errors might be done away or corrected; and as I was frequently fortunate enough to carry on my ſurveys in this manner for weeks together, inſtances that might corroborate or contradict the preceding remarks are neither very numerous or pointed; the following are the moſt remarkable:

April 15th, AM 11h 50′	Π weſtern part	- -	N 15° E
	A peaked hummock	- -	N 20 E
—	Furtheſt extreme from deck	-	S 54 E
—	Centre of a naked ſandy patch	-	E 0½ S.

In the ſame manner upon ſingle ſets of bearings I was obliged to allow a variation different from what I ſuppoſed the true to be, unleſs the ſhip's head was nearly north or ſouth: but, that I might proceed as little upon conjecture as poſſible, I always endeavoured to get obſervations for the variation when the ſhip's head was in the ſame direction as when I had taken or wiſhed to take a particular ſet of bearings, and I then allowed that variation exactly, whatever it was. The perplexity ariſing from diſagreements in bearings was by theſe means much alleviated, and happy agreements were frequently produced, when, without ſuch corrections, there was nothing but diſcord.

TABLE

TABLE of observed Variations of the Compass, and of the Influence of the Ship's Position upon them.

Time.	Latitude.	Longitude.	Number of compasses used.	Number of sets of observations taken.	Place of the compass.	Supposed true variation.	Observed variation.	Ship's head.	Observer.
1802.									
Aug. 5, PM	23° 51' S	151° 42' E	one	1 amplitude	binnacle	8° 0' E	12° 7' E	WSW	Commander
— AM	23 51	151 40	—	—	—	—	10 15	WNW	
12, PM	23 30	151 11	three	6 azimuths	—	—	6 50	SSE	Lt. Flinders
18, PM	23 23	151 16	one	2 —	—	7 45	7 52	W	Commander
31	22 23	150 38	two	4 —	—	7 30	4 49	E	
Sept. 6, AM	Upon Pier	Head	theodolite	1 —	on shore	8 0	8 2	—	
Oct. 14, PM	20 44	150 42	one	1 amplitude	binnacle	7 0	6 40	SSE	Lt. Flinders
20, PM	19 22	148 40	—	1 —	—	6 0	5 39	S	Commander
21, AM	18 15	148 38	three	6 azimuths	—	—	5 42	N b. E	Lt. Flinders
Nov. 2, PM	10 30	142 32	one	2 —	—	4 0	3 32	E	Commander
7, AM	12 11	142 0	—	2 —	—	—	4 4	S	Lt. Flinders
9, PM	12 37	142 2	—	1 amplitude	—	—	5 24	W	Commander
1803.									
Jan. 3, PM	14 20	136 16	—	1 —	—	2 30	0 58	E	
7, PM	14 20	136 37	—	1 —	—	—	1 9	SE	
13, {PM AM}	13 38	137 20	—	2 —	—	3 0	3 47	Westerly	Lt. Flinders
14, AM	13 35	136 58	—	1 —	—	—	5 51	WSW	Commander
16, PM	In NW Bay	(Gr. Eyl.)	theodolite	1 azimuth	on shore	—	3 6	—	
Feb. 3, AM	Arnhem S Bay		three	6 —	binnacle	2 20	2 26	NW b. W	
9, AM	—	—	theodolite	1 —	on shore	—	2 20	—	
Mar. 10, AM	11 5	134 15	one	2 —	binnacle	1 0	1 55	WNW	

In.

In the latter of these observations, the differences arising from a change in the direction of the ſhip's head is leſs conſiderable than in the higher latitudes; indeed, on approaching the line of no variation upon the ſouth coaſt, the differences in the variation were ſmaller than before and afterwards; but that theſe differences ſhall be greater in a large variation and ſmaller in a leſs, both places being equally diſtant from the magnetic pole, I will not venture to aſſert. The inferences that I think may be ſafely drawn from the above obſervations are as follows: 1ſt. That there was a difference in the direction of the magnetic needle on board the Inveſtigator when the ſhip's head pointed to the eaſt, and when it was directed weſtward. 2d. That this difference was eaſterly when the ſhip's head was pointed to the weſt, and weſterly when it was eaſt. 3d. That when the ſhip's head was north or ſouth the needle took the ſame direction or nearly ſo that it would on ſhore; and ſhewed a variation from the true meridian, which was nearly the medium between what it ſhowed when eaſt and when weſt. 4th. That the error in variation was nearly proportionate to the number of points which the ſhip's head was from the north or ſouth. Conſtant employment upon practice has not allowed me to become much acquainted with theories, but the little information I have upon the ſubject of magnetiſm has led me to form ſome notion concerning the cauſe of theſe differences, and although moſt probably vague and unſcientific, I truſt for the candour of the learned in ſubmitting it, as well as the inferences above drawn, to their judgment.

1ſt. I ſuppoſe the attractive power of the different bodies in a ſhip, which are capable of affecting the compaſs, to be collected into ſomething like a focal point or center of gravity, and that this point is nearly in the center of the ſhip where the ſhot are depoſited, for here the greateſt quantity of iron is collected together.

2d. I ſuppoſe this point to be endued with the ſame kind of attraction as the pole of the hemiſphere where the ſhip is; conſequently, in New Holland the ſouth end of the needle would be attracted by it and the north end repelled.

3d. That the attractive power of this point is ſufficiently ſtrong in a ſhip of war to interfere with the action of the magnetic

General inferences. 1. The compaſs was found to deviate accordingly as the ſhip's head was eaſterly or weſterly; the north end being farther from the ſhip's head than the truth. The deviation was proportional to the diſtance of the head from the N. or S.

Theory propoſed. That all the iron in the ſhip acts like one magnet, having a different polarity accordingly as the ſhip is near the N. or S. pole of the terreſtrial magnetiſm.

magnetic poles upon a compaſs placed upon or in the binnacle.

If theſe ſuppoſitions are conſiſtent with the laws of magnetiſm, eſtabliſhed by experiments, I judge that they will account for all the differences above noticed; for the interference will neceſſarily be moſt perceptible upon a compaſs when the attractive point is at right angles to the magnetic meridian, that is, when the ſhip's head is eaſt or weſt, and will altogether vaniſh or become imperceptible when the attractive point and meridian coincide, or when the ſhip's head is north or ſouth. That the power of this point ſhould become leſs as the ſhip increaſes her diſtance from the magnetic pole has not indeed entered into my ſuppoſitions; but it may probably be true, and is indeed almoſt a neceſſary conſequence of the ſecond ſuppoſition. If the above hypotheſis, ſo to call it, be true, it muſt follow, that the differences in the variation of the magnetic needle, ariſing from a change in the ſhip's head, ought to be directly contrary to thoſe before recited, when the ſhip is on the north ſide of the magnetic equator, for the north point of the needle ſhould then be attracted, and the ſouth end repelled. I have no obſervations which are very deciſive upon this head, but thoſe that were taken on board the Inveſtigator ſeem to beſpeak that as it is ſo; they are as follow.

Inferences from the laſt ſuppoſition: that the effects ſhould have a contrary direction in north latitude.

TABLE

TABLE of Obfervations to illuftrate the foregoing Inferences.

Time.	Latitude.	Longitude.	Number of compaffes ufed.	Number of fets of obfervations taken.	Place of the compafs.	Suppofed true variation.	Obferved variation.	Ship's head.	Obferver.
1801.									
July 21, PM	Start Point in fight to the NE		two	5 azimuths	binnacle	—	29° 34′ W	W	Mr. Thiftle
—			one	1 amplitude	—	—	29 30	—	—
22, PM	49° 10′ N	5° 25′ W	two	4 azimuths	upon the booms in	—	24 12	WNW	—
—, AM	48 15	6 45	one	1 amplitude	the middle	—	24 49	WSW	—
28, PM	38 1	14 20	five	10 azimuths	of the fhip	—	20 57	SW	—
—	—	—	—	11 —	binnacle	—	25 34	—	Commander
31, PM	Porto Santo in fight to the NW		two	4 —	—	—	22 45	—	Mr. Thiftle
—			—	4 —	booms	—	19 51	—	—
Aug. 24, AM	10 20	22 15	one	2 —	binnacle	—	12 45	SE b. S	Commander
29, AM	5 40	16 30	two	4 —	—	—	12 18	—	Lt. Flinders
Sept. 5, AM	2 15	14 00	—	3 —	—	—	14 54	WSW	Mr. Thiftle

Thefe

These obfervations, particularly thofe of July 28, feem to be decifive in fhowing that the variation is more wefterly when taken upon the binnacle of a fhip whofe head is weftward in north latitude, than when obferved in the center of the fhip, which is a ftrong confirmation of the fuppofitions before given; but the obfervations on the change of the fhip's head are too few to be fatisfactory. Almoft every fea officer can tell whether he has obferved the variation of the compafs to be greater when going down the Englifh Channel than when coming up it: and indeed it would be very eafy for a fhip lying in harbour to afcertain the point beyond controverfy.

Remarks and obfervations on the fame law.

Should this point be well eftablifhed, I think it would follow, that from a high fouth latitude where the differences are great on one fide, they are moft likely to decreafe gradually to the equator, and to increafe in the fame way to a high north latitude, where they are great on the other fide; thus the fmaller differences on the north coaft of New Holland will be accounted for. I fhall leave it to the learned on the fubject of magnetifm to compare the obfervations here given with thofe made by others in different parts of the earth, and to form from them an hypothefis that may embrace the whole of the phenomena: the opinion I have ventured to offer is merely the vague conjecture of one who does not profefs to underftand the fubject.

Inftances of the compafs being affected by local magnetifm.

Some account of the magnetifm of Pier Head, upon the eaft coaft of New Holland, may not perhaps be thought an unappropriate conclufion to this Paper. I was induced to attend to this from the following paffage in Hawkesworth, Vol. III. p. 123. "At fun-rife I went afhore," fays Captain Cook, "and climbing a confiderable hill," Pier Head, "I took a view of the coaft and the iflands that lie off it, with their bearings having an azimuth compafs with me for that purpofe; but I obferved that the needle differed very confiderably in its pofition, even to thirty degrees, in fome places more, in others lefs; and once I found it differ from itfelf no lefs than two points in the diftance of fourteen feet*. I took up fome

Pier Head.

Local deviation of the compafs as far as 50°.

* In a fet of angles taken near the head of Arnhem north bay, on the weft fide of the gulph of Carpentaria, I found the needle of the theodolite had been drawn 50° from its proper direction. The fhore confifted of grains of iron ore caked into a ftony mafs; and a piece of it, when applied to the needle, drew it fix or eight degrees

some of the loose stones that lay upon the ground, and applied them to the needle, but they produced no effect; and I therefore concluded that there was iron ore in the hills, of which I had remarked other indications, both here and in the neighbouring parts."

On landing at Pier Head I found the stones lying on the surface to be porphyry, of a dark bluish colour; but although I understand this species is usually found to possess some magnetic power, a piece did not produce any sensible effect upon the needle of the theodolite when applied to it. In the following observations the theodolite always stood about four feet from the ground, that being nearly the length of its legs. I first took an extensive set of bearings from the top of the hill, amongst which were two stations whence Pier Head had been before set. The first, called Extensive Mount, distant 34 miles, differed from its back bearing 4° 35′ to the right, and the second, island *a*, distant 29½ miles, differed 4° 45′ the same way. I now moved the theodolite three yards to the westward, and the same two objects bore 2° 10′ to the right of their back bearing; on moving it three yards to the south-eastward from the first place, they differed 2° to the left; and on moving the theodolite four yards to the northward, the same two objects bore 1° 10′ to the right of their back bearings. On the following morning I determined to try the magnetism more particularly. Taking the theodolite and dipping-needle, I landed upon the shore of the Head, whence the top of the hill bore N 50° W, about one-third of a mile. The variation of the

The author's obs. at Pier Head.

degrees from its direction, but it then swung back to its error of 50° where it was stationary. In Arnhem south bay a small piece of similar stone drew the needle of the theodolite entirely round, yet the bearings taken in this place did not show any disagreement from the variation and bearings taken in the neighbouring places, where the stone did not produce any such effect. In most places on shore, where I had occasion to take angles, it was my practice to try the effect of a piece of the stone upon the theodolite, in order to detect the presence of iron ore, as well as on account of my survey. It commonly happened that no effect was apparent, but yet I could not trust implicitly to the angles, (particularly on the main land,) unless observations for the variation were taken before the instrument was moved, or I had a back bearing of some station where such observations had been made.

The author's obs. at Pier Head.

theodolite in this place I observed to be 8° 2′ E, and the inclination of the south end of the dipping needle 50° 50′, the needle stood vertical when the face of the instrument was S 2° E. I then took the following bearings: Extensive Mount 108° 30′, the same exactly as by back bearing. Double Peak 143° 30′; from hence I rowed round the Head, and landed on a rock, whence the top of the hill bore SSW one-fixth of a mile; Extensive Mount bore 110° 14′, the inclination of the dipping-needle 50° 29′, and the needle stood vertical when the instrument faced S 3° E. Thus the difference was 1½° in the horizontal, and ½° in the vertical direction of the needle. Ascending the hill, I made the following observations on the top: Extensive Mount 113° 50′, *a* island 133° 52′, Double Peak 148° 32′; the inclination of the needle was 53° 20′, and is stood vertical at S 3° E. The differences here are 5° 10′ in the horizontal, and 2° 30′ in the vertical direction, from what the needle stood at in the first morning's place. On moving ten yards SSE, the bearings were, Extensive Mount 108° 44′, Double Peak 143° 25′; the inclination was 52° 18′, and the needle was vertical when the instrument faced S 5° W. In this 4th set of observations, the horizontal direction of the needle is only a few minutes different from the first place, but the vertical direction is 1° 28′. From the top of the hill I now moved twenty yards to the north-eastward, when Extensive Mount bore 110°, Double Peak 144° 42′; the inclination of the dipping needle was now 50° 35′, and it stood vertical at S 3° W. Thus it appears that the polarity of the magnetic needle is most interrupted at the top of the hill, both according to the theodolite and dipping-needle. Whether this may arise from some particular magnetic substance lodged in the heart of the hill, or from the attractive powers of all the substances which compose Pier Head being centered in a similar point to what I have supposed to take place with all the ferruginous bodies lodged within a ship, I shall not attempt to decide. The greater differences in the horizontal direction of the needle observed by Captain Cook, might have arisen from his using a common azimuth compass, which was probably not further elevated from the ground than to be placed on a stone.

MATTHEW FLINDERS.

Isle of France,
March 5th, 1804.

IV.

Letter from Mr. Robert Harrup, *ſhewing that the Smut in Wheat exiſts in the Seed, and is greatly remedied by Lime ſteeping.*

To Mr. NICHOLSON,

SIR, *Chobham, January* 7, 1806.

LITTLE converſant in agricultural affairs, I am yet to learn what enquiries have been made into the nature and cauſes of the diſeaſes of grain.

If the following communication on the diſeaſe of wheat, known by the name of *ſmut*, contains any thing new, or may lead to farther inveſtigation; an early inſertion of it will greatly oblige,

Sir,

Your obedient humble Servant,

R. HARRUP.

Different cauſes have been aſſigned for the production of ſmut; ſome ſuppoſing it to ariſe from too great an abundance of water ſhoots, others from intemperate ſeaſons. Cauſes uſually aſſigned for the ſmut.

A writer in a reſpectable publication ſtrenuouſly contends in favour of the latter opinion.

He informs us, that brine, pickling, liming, change of ſeed, and ſeed of one year old, and upwards, avail nothing. In cold wet ſummers, ſays he, the ſmut prevails notwithſtanding the uſe of every means which invention hath urged or ingenuity practiced. After a number of obſervations, he continues, "to ſum up the whole of this matter, it ſeems as certain as demonſtration can render it, that the ſmut is not owing to any defect or imperfection in the ſeed, but entirely to ſome corrupt-creating principle in the atmoſphere, in the blowing ſeaſon, which blights and deſtroys the grain in ſome ſhape or other, according to the time it has been blowing, when it is ſtruck with the blight." Intemperate weather: and that attention to the ſeed avails nothing.

On the contrary, it would appear from the accounts of thoſe who have the moſt frequent opportunities of making obſervations, that the primary cauſe of ſmut is in the ſeed. All the farmers I have converſed with on the ſubject, are decidedly Practical men uſually aſcribe it to the ſeed,

decidedly of opinion, that ſmut in the ſeed will produce ſmut in the crop, unleſs certain means are uſed to prevent it. With this intention I have ſomewhere ſeen a variety of preparations recommended, in ſome of which arſenic was one of the ingredients. The farmers in this neighbourhood prepare their ſeed wheat in one or other of the following methods.

and uſe preparations.

Steeping in brine:

Formerly the wheat was immerſed about twelve hours in a ſtrong ſolution of common ſalt in water, and afterwards dried by mixing it with a ſufficient quantity of lime newly ſlaked.

wetting,

Of late years, in place of immerſing it, they pour a quantity of the ſaline ſolution over it on the floor, and after mixing the whole well together, dry it with lime as before.

or treatment with lime water only.

Another method which is now pretty generally adopted, is that in which no ſalt is uſed.

A quantity of boiling water is poured upon quick lime, and kept conſtantly ſtirred till the lime is reduced to powder, when it is immediately mixed with the grain. No great accuracy is uſed in aſcertaining the proportions; five or ſix pounds of lime, and three gallons of boiling water are about ſufficient to prepare five buſhels of wheat. In reaſoning *a priori*, one would be apt to ſuppoſe, that the vegetative powers of the grain would be materially injured by this boiling compoſition, but experience proves the contrary.

Experiment. Equal meaſures of ſound and ſmutty wheat were mixed. Half of this was ſteeped in brine for twelve hours, and half left unprepared.

Amidſt this diverſity of opinion on the cauſe of ſmut, I wiſhed to aſcertain the truth, if poſſible, by experiment. Accordingly, ſo early as December 1798, I mixed intimately together equal meaſures of ſound wheat and grains conſiſting entirely of ſmut. The heap was then divided into two equal parts; one of them was put into a ſaturated ſolution of ſalt in water for twelve hours, and then mixed with quick lime. The other part was ſubjected to no preparation whatever. Five or ſix days afterwards they were both planted in drills on a ſouth border, about nine or ten yards apart. Both parcels came up about the ſame time, and while in blade, no difference could be perceived. While the ear was yet inveloped by the blade, I cautiouſly opened ſeveral of both crops, and in ſome of that which had undergone no preparation, a conſiderable difference was obſervable. Some of the embrio grains were opened, and in place of a milky juice, they contained only a ſmall quantity of a whitiſh ſubſtance, in which,

Both parcels were planted.

by

by the help of a common magnifier, I could readily distinguish from one, to three or four black specks in each. When rubbed between the fingers, a faint smell of smut was emitted. The ears which were examined, and had this appearance were marked, and afterwards proved to be smut. When the crops came out in ear, it was easy to distinguish the smut from the wheat. At the time of blowing no blossom whatever appeared on the smut ears, and the weather proving tempestuous at that time, the blossom was frequently washed off the wheat ears by heavy showers, and as often renewed.

The unprepared seed gave an unhealthy product.

Smut easily distinguished.

Both pieces were cut at the usual time, and upon a careful examination, *that which had been subjected to no preparation consisted of nearly two-thirds of smut ears, the remainder being tolerably good wheat. In that which had been prepared, not a single smut-ball could be found.*

The prepared seed produced sound grain, and the unprepared seed smutty grain.

An accidental occurrence may be mentioned in corroboration of this experiment. Happening to pass through a small field of wheat just before the commencement of the harvest, I was struck with the unusual quantity of smut in one part of it. On close examination, I found that this extraordinary crop of smut ended abruptly in a line along one of the furrows. The other parts of the field had much the same appearance of others in the neighbourhood; a few smut ears scattered through it. Upon enquiry I found, that the seed with which this field had been sown, running short, the piece so abundant in smut had been sown in seed which contained a considerable quantity of smut, and had undergone no preparation, only sprinkling it with a little slaked lime immediately before sowing.

An accidental occurrence of much smut confined to a part of a field, which had been sown with unprepared seed.

The disease of smut is entirely confined to the grain. The straw and every other part of the plant is sound, and arrives at the natural size. Smut ears are *staring*, and of a dirty whitish colour, inclining to blue, at the time when healthy ears are of a bright yellow. Their odour is fœtid, and not inaptly compared to that of stale lobsters. Part of an ear is not unfrequently found to contain smut, while the other parts are filled with sound wheat.

Smut affects the grain only.

Description of the smut.

Diseased grains have more the globular form than those of sound wheat, which is perhaps the reason why they are called smut-balls. The skin is shrivelled and of a dirty brownish

hue

hue, without any perforations which can be difcovered by a high magnifying power. The whole of their contents, in a recent ftate, are a blackifh foft fubftance with a few fhining fpecks, which difappear when dried.

The duft of fmut confifts of globules,

When kept fome time in a dry place, this foft fubftance is in the form of a fine duft or powder, of a dark brown colour when fpread out on glafs or talc. The microfcope fhews each of thefe minute particles to be well formed globules, fomewhat larger than the fanguineous.

heavier than water:

They are fpecifically heavier than water with which they readily mix but foon fubfides, fuffering no change by being kept in that fluid. In the beginning of September laft, I infufed fome of the powder in water in a watch-glafs. A few hours after I difcovered by the microfcope, in a drop of the fluid a few animalculæ. Upon examination next day every drop of the liquor contained innumerable animalculæ, generally very minute but fome a fize larger. After ftanding expofed fome days, the water evaporated, and an hour or two after the addition of frefh water every part fwarmed with animalculæ, moving nimbly in all directions. While viewing them in the microfcope they fuddenly became motionlefs owing to the evaporation of the drop of liquid; on adding a drop of frefh water, they inftantly revived and began the fame lively motion. A quantity of falt fufficient to faturate the water was then added to the mixture. Upon examination about twenty hours afterwards, I was much furprized to find the animalculæ as numerous and lively as before the addition of the falt.

in which they produce animalcules,

which are not killed by falt;

The watch glafs with its contents, after ftanding neglected, on a fhelf expofed to the effluvia of a variety of drugs, till the latter end of November, was again filled with water, and placed near a fire, placing at the fame time by it a fimilar glafs, containing fmut powder and frefh water. They were both frequently examined for fome days, but without difcovering any animalculæ. My attention being called off by other avocations they remained unnoticed about eight days. The glafs which contained the infufion with fimple water was quite dry, and only a fmall quantity of fluid remained in the other. A drop being examined in the microfcope by a fingle lens of a high magnifying power, was found

to

to swarm with animalculæ. Both glasses were now filled with fresh water, and placed under inverted jars. Being examined two days after, each of them swarmed with lively animalculæ. While viewing them, *a small particle of lime water was added to the drop, which proved instantly fatal*, at least all motion ceased instantaneously, and was not renewed. Lime water killed the animalcules from smut.

Among other inferences which may be drawn from the preceding facts and observations, are first, that the cause of smut is in the seed, and that smut produces smut in the crop. At the same time it is readily admitted, that certain seasons are more favourable to smut than others, which can only be considered as a secondary cause. 2. That lime used in the manner above mentioned, prevents smut, if not entirely, at least so far as not to prove injurious. Inference. Smut is caused by bad seed, and lime water prevents it.

Is smut occasioned by animalculæ? Some of the foregoing facts seem strongly to favour the idea *.

V.

On the Discovery of Palladium; with Observations on other Substances found with Platina. By WILLIAM HYDE WOLLASTON, *M. D., Sec. R. S* †.

HAVING some time since purified a large quantity of platina by precipitation, I have had an opportunity of observing various circumstances in the solution of this singular mineral, that have not been noticed by others, and which, I think, cannot fail to be interesting to this Society. The principal subject of the present memoir is palladium.

* Mr. Nicholson will readily perceive that the subject is not near exhausted. If future investigation should present any thing worthy of communication on the subject, should Mr. N. deem such deserving a place in the Philosophical Journal, he has only to mention it in a marginal line.

Answer. The diseases of corn form a subject of such high importance, whether considered in an economical or scientific point of view, that I must consider it a duty to pay the most marked attention to whatever may tend to elucidate it.—N.

† Philos. Trans. 1805, p. 316.

As

As I have already given an account * of one product obtained from that ore, which I conſidered as a new metallic ſubſtance, and denominated Rhodium, I ſhall on the preſent occaſion confine myſelf principally to thoſe proceſſes by which I originally detected, and ſubſequently obtained another metal, to which I gave the name of *Palladium*, from the planet that had been diſcovered nearly at the ſame time by Dr. Olbers.

In the courſe of my inquiries I have alſo examined the many impurities that are uſually mixed with the grains of platina, but I ſhall not think it neceſſary to deſcribe minutely ſubſtances which have already been fully examined by others.

§ I. *Ore of Iridium.*

Ore of iridium, reſembles that of platina, but is inſoluble in nitro-muriatic acid; grains, harder to the file, not malleable and peculiar in their fracture:

I muſt however notice one ore, that I find accompanies the ore of platina, but has paſſed unobſerved from its great reſemblance to the grains of platina, and on that account is ſcarcely to be diſtinguiſhed or ſeparated from them, excepting by ſolution of the platina; for the grains of which I ſpeak are wholly inſoluble in nitro-muriatic acid. When tried by the file, they are harder than the grains of platina; under the hammer they are not in the leaſt degree malleable; and in the fracture they appear to conſiſt of laminæ poſſeſſing a peculiar luſtre; ſo that although the greater number of them cannot, as I have before obſerved, be diſtinguiſhed from the grains of platina, the laminated ſtructure ſometimes occaſions an external form by which they may be detected. With a view to be abſolutely certain that there exiſt grains in a natural ſtate, which have not been detached by ſolution from the ſubſtance of the grains of platina, I have ſeparated from the mixed ore as many as enabled me to aſcertain their general compoſition.

much heavier than the grains of platina;

Their moſt remarkable quality is their great ſpecific gravity, which I have found to be as much as 19,5, while that of the crude grains of platina has not, in any experiment that I have made, exceeded 17,7. From this circumſtance it might naturally be conjectured that they contain a greater quantity of platina than the grains in general; by analyſis, however, they do not appear to me to contain the ſmalleſt quantity of that metal, but

of which metal they contain none.

* See our Journal, IV. 107.

to be an ore consisting entirely of the metals that were found by Mr. Tennant in the black powder which is extricated by solution from the grains of platina, and which he has called Iridium and Osmium. But, since the specific gravity of these grains so much exceeds that of the powder, which by my experiments has appeared to be, at the utmost, 14,2. I have thought it might deserve inquiry whether their chemical composition is in any respect different. For this purpose I have selected a portion of them, and have requested Mr. Tennant to undertake a comparative examination, from whose well known skill in chemical inquiries, as well as peculiar knowledge of the subject, we have every reason to expect a complete analysis of this ore.

§ II. *Hyacinths.*

Among those bodies which may be separated from the ore of platina, in consequence of their less specific gravity, by a current of water or of air, there may be discerned a small proportion of red crystals so minute, that 100 of the largest I could collect weighed scarcely $\frac{3}{10}$ of a grain. The quantity which I possess is consequently too small for chemical analysis; but their physical properties are such as correspond in every respect with those of the hyacinth. I was first led to compare them with that stone by their specific gravity, which I conjectured to be considerable from their accompanying other substances, that appear to have been collected together solely by reason of their superior weight. — Very small hyacinths found among the platina grains;

Like the hyacinth, these crystals lose their colour immediately and entirely when heated; they also agree with it in their hardness, which is barely sufficient to scratch quartz, but is decidedly inferior to that of the topaz.

The principal varieties of their form may be very well understood by description.

1st. In its most simple state the crystal may be considered as a rectangular prism terminated by a quadrilateral obtuse pyramid, the sides of which sometimes arise direct from the sides of the prism; but, — Varieties of their forms;

2dly. The position of the pyramid is generally such that its sides arise from the angles of the prism. In this case the sides of the prism are hexagons.

3dly. It

3dly. It is more ufual for the prifm to have eight fides by truncation of each of its angles, and at each extremity eight additional furfaces occupying the place of the eight linear angles between the prifm and terminating pyramid of the fecond variety. The complete cryftal has then thirty-two fides.

4thly. The eight furfaces laft mentioned, as interpofed between the prifm and pyramid, are fometimes elongated into a complete acute pyramid having eight fides arifing from the angles of an octahedral prifm.

which prove the nature of the ftone.

The third form above defcribed, correfponds fo entirely with that given by the Abbé Haüy * as one of the forms of the hyacinth or jargon, that I have little reafon to regret my inability to obtain chemical evidence of the compofition of thefe cryftals.

Thofe, and other impurities, I ufually feparated, as far as was practicable, by mechanical means, previoufly to forming the folution of platina, which has been the principal object of my attention.

§ III. *Precipitation of Platina.*

Account of the treatment of platina. After the firft precipitation of it by fal ammoniac: another portion was thrown down by iron.

When a confiderable quantity of the ore has been diffolved, and I had obtained, in the form of a yellow triple falt, as much of the platina as could be precipitated by fal ammoniac, clean bars of iron were next immerfed in the folution for the purpofe of precipitating the remainder of the platina.

This is called the firft metallic precipitate, and was again diffolved as the firft ore had been, and again precipitated by fal ammoniac.

For diftinction it will be convenient to call this, which in fact confifts of various metals, the firft metallic precipitate. The treatment of this precipitate differed in no refpect from that of the original ore. It was diffolved as before, and a portion of platina precipitated by fal ammoniac; but it was obfervable that the precipitate now obtained was not of fo pale a yellow as the preceding. Neverthelefs the impurity was in fo fmall quantity, that the platina reduced from it by heat did not differ difcernibly from that obtained from the pureft yellow precipitate.

* *Traité de Mineralogie*, Pl. XLI. fig. 17. *Journ. des Mines*, No. 26, fig. 9.

At

At this time I found it advantageous to neutralize the solution with soda, and to employ a solution of green sulphate of iron for the precipitation of the gold, of which, I believe, a portion may always be obtained from the mixed ore; but I have observed in experiments upon any quantities of mere grains of crude platina carefully selected, that the smallest portion of gold could not be detected as a constituent part of the ore itself.

The solution was neutralized with soda; and gold, if present, precipitated by solution of green sulphate of iron.

Bars of iron were subsequently employed as before for recovering the platina that remained dissolved, together with those substances which I have since found to accompany it.

A second metallic precipitate was thrown down by iron.

The precipitate thus obtained, which I distinguish by the name of the second metallic precipitate, was to appearance of a blacker colour than the former, and was a finer powder.

As I was not at first prepared to expect any new bodies, I proceeded to treat the second precipitate, as the former, by solution and precipitation. But I soon observed appearances which I could not explain by supposition of the presence of any known bodies, and was led to form conjectures of future discoveries, which subsequent inquiry has fully confirmed.

When I attempted to dissolve this second metallic precipitate in nitro-muriatic acid, I was surprised to find that a part of it resisted the action of that solvent, notwithstanding any variations in the relative proportions or strength of the acids employed to form the compound, and although the whole of this powder had certainly been twice completely dissolved.

This was not all soluble in nitro-muriatic acid.

The solution formed in this case was of a peculiarly dark colour, and when I endeavoured to precipitate the platina from it by sal ammoniac, the precipitate obtained was small in quantity, and, instead of being yellow, was of deep red colour, arising from an impurity which I did not at that time understand, but which we since know, from the experiments of Mr. Descotils, is occasioned by the metal now called iridium.

This solution was very dark and its precipitation by sal ammoniac was deep red, occasioned by iridium.

The solution, instead of being rendered pale by the precipitation of the platina, retained its dark colour in consequence of the other metals that remained in solution; but, as I had not then learned the means of separating them from each other, and as the quantity of fluid which accumulated occasioned me some inconvenience, I decomposed it by iron, as

Precipitation of a third metallic precipitate by iron.

in

in the former inftances, and formed a third metallic precipitate, which could more commodioufly be referved for fubfequent examination.

Much of this being rhodium, was infoluble.

In this laft ftep I committed an error which afterwards occafioned me confiderable difficulty, for I found that a great part of this precipitate confifting of rhodium was unexpectedly rendered infoluble by this treatment, and refembled the refiduum of the fecond metallic precipitate abovementioned.

As I have already communicated to this fociety, in my Paper upon rhodium, the procefs by which I fubfequently avoided this difficulty, I fhall at prefent return to a previous ftage of my progrefs, and relate the means by which I firft obtained palladium in my attempts to analyze the fecond metallic precipitate.

§ IV. *Separation of Palladium.*

Separation of palladium. The fecond metallic precipitate contained lead, copper, and another metal precipitable by copper.

There was no difficulty in afcertaining the prefence of lead as one of the ingredients of this precipitate, by means of muriatic acid, which diffolved lead and iron and a fmall quantity of copper. It was equally eafy to obtain a larger portion of copper by dilute nitrous acid, with which it formed as ufual a blue folution. But when I endeavoured to extract the whole of the copper by a ftronger acid, it was evident, from the dark brown colour of the folution, that fome other metallic ingredient had alfo been diffolved. I at firft afcribed this colour to iron; but, when I confidered that this fubftance had been more flowly acted upon than copper, I relinquifhed that hypothefis, and endeavouring to precipitate a portion of it by a clean plate of copper, I obtained a black powder adhering to a furface of platina on which I had placed the folution. As this precipitate was foluble in nitric acid, it evidently confifted neither of gold nor platina; as the folution in that acid was of a red colour, the metal could not be either filver or mercury; and as the precipitation of it by copper excluded the fuppofition of all other known metals, I had reafon to fufpect the prefence of fome new body, but was not fully fatisfied of its exiftence until I attempted the precipitation of it by mercury.

It was feparated by agitating mercury with the folution with

For this purpofe I agitated a fmall quantity of mercury in the nitrous folution previoufly warmed, and obferved the mercury to acquire the confiftence of an amalgam. After this

amalgam had been exposed to a red heat, there remained a white metal, which could not be fused before the blowpipe. It gave a red solution as before in nitrous acid; it was not precipitated by sal ammoniac, or by nitre; but by prussiate of potash it gave a yellow or orange precipitate; and in the order of its affinities it was precipitated by mercury but not by silver.

which it formed an amalgam; and the mercury was driven of by heat. It was palladium.

These are the properties by which I originally distinguished palladium; and by the assistance of these properties I obtained a sufficient quantity for investigating its nature more fully.

There were, however, various reasons which induced me to relinquish the original process of solution in nitrous acid and precipitation by mercury; for although I found the metal thus obtained to be nearly pure, the necessity of agitating the solution with the mercury was very tedious, and the waste was also considerable; for in the first place it seemed that nitrous acid would not extract all the palladium from any quantity of the second metallic precipitate, neither would mercury reduce the whole of what was so dissolved. I therefore substituted a process dependent on another of its properties, I had observed that this metal differed from platina in not being precipitated from nitro-muriatic acid by nitre or by other salts containing potash; for although a triple salt is thus formed, this salt is extremely soluble, while that of platina on the contrary requires a large quantity of water for its solution. On that account a compound *menstruum* consisting of nitrate of potash dissolved in muriatic acid is unfit for the solution of platina, but dissolves palladium nearly as well as common nitro-muriatic acid in which there is no potash present *.

The process with mercury was abandoned;

In five ounces of muriatic acid diluted with an equal quantity of water, I dissolved one ounce of nitre, and formed a solvent for palladium that possesses little power of acting on platina, so that by digesting any quantity of the second metallic precipitate till there appeared to be no farther action, I procured a solution from which by due evaporation were formed crystals of a triple salt, consisting of palladium combined with muriatic acid and potash. These are the crystals which I have on a former

and a solvent consisting of muriatic acid, with nitre was used to the second precipitation from which it takes palladium but not platina.

The solution gave crystals of triple salt of palladium potash and muriatic acid.

* I have found that gold may also be dissolved with equal facility by the same solvent, and nearly in the same proportion. Ten grains of nitre added to a proper quantity of muriatic acid are sufficient for sixteen grains of either gold or palladium.

former occasion * mentioned as exhibiting a very singular contrast of colours, being bright green when seen tranversely, but red in the direction of their axis; the general aspect, however, of large crystals is dark brown.

From the salt thus formed and purified by a second crystallization, the metal may be precipitated nearly pure by iron or by zinc, or it may be rendered so by subsequent digestion in muriatic acid.

§ V. *Reasons for thinking Palladium a simple Metal.*

That palladium is a simple metal appears from,

From the consideration of this salt alone I thought it highly probable that the substance combined in it with muriate of potash was a simple metal, for I know of *no instance in chemistry of a distinctly crystallized salt containing more than two bases combined with one acid.* I nevertheless endeavoured by a suitable course of experiments to obviate all probable objections.

its forming a distinctly crystallized salt with bases and an acid,

After examining by what acids it might be dissolved and by what reagents it might be precipitated, I combined it with various metals, with platina, with gold, with silver, with copper, and with lead; and when I had recovered it from its alloys so formed, I ascertained that, after every mode of trial it still retained its characteristic properties, being soluble in nitrous acid, and precipitable from thence by mercury, by green sulphate of iron, by muriate of tin, by prussiate of potash, by each of the pure alkalis, and hydrosulphurets.

its combinations with metals and separation without change.

and its precipitation is reducible by mere heat.

The precipitate obtained in each case was also found to be reducible by mere heat to a white metal, that, except in very small quantities, could not be fused alone by the blowpipe, but could very readily be fused with sulphur, with arsenic, or with phosphorus, and in all other respects resembled the original metal.

Qu. Whether it might consist of metal and a fixed acid?

The only hypothesis, on which I thought it possible that I could be deceived, arose from the recollection of the error, which subsisted for a few years, respecting the compound formerly called siderite. It was possible that some metallic or other fixed acid might unite too intimately with either a known or an unknown metal to be separated by the more common simple affinities. I consequently made such attempts as appeared best calculated to disunite a compound so constituted.

* Phil. Trans. 1804, p. 428.

Having

Having boiled the oxide with pure alkalis, and found it to be unaltered, I thought the affinities of lime or lead might be more likely to detect the presence of the phosphoric or of any known metallic acid; and accordingly I made various attempts by muriate and nitrate of lime, as well as by nitrate of lead, to effect a decomposition of the supposed compound. In the experiment on which I placed the greatest reliance, I poured liquid muriate of lime into a solution of palladium in nitro-muriatic acid, and evaporated the mixture to dryness, intending thereby to expel any excess of acid that might have been left in the solution, and to render either phosphate of lime, or any compound of lime with a metallic acid, insoluble in water. The residuum however was very readily dissolved by water, and consisted merely of muriate of lime and muriate of palladium, without any appearance of decomposition.

The oxide is not affected by boiling with alkalis; nor by pouring muriate of lime into its solution.

When I found all my endeavours directed to that end wholly unsuccessful, I no longer entertained any doubt of this substance being a new simple metal, and accordingly published a concise delineation of its character; but by not directing the attention of chemists to the substance from which it had been extracted, I reserved to myself an opportunity of examining more at leisure many anomalous phenomena, that had occurred to me in the analysis of platina, which I was at a loss to explain, until I had learned to distinguish those peculiarities, that I afterwards found to arise from the presence of rhodium.

Hence the discoverer was warranted in publishing it as a new metal.

§ VI. *Additional Properties of Palladium.*

In my former Paper on that subject I also added some observations upon the properties and origin of palladium, describing only such a mode of obtaining it from platina as should avoid the introduction of any unnecessary ingredient which might possibly be misinterpreted, and omitted one of the most distinguishing properties of palladium, by means of which it may be obtained with the utmost facility by any one who possesses a sufficient quantity of the ore of platina.

Method of easily separating palladium.

To a solution of crude platina, whether rendered neutral by evaporation of redundant acid, or saturated by addition of potash, of soda, or ammonia, by lime or magnesia, by mercury, by copper, or by iron, and also whether the platina has or has not been precipitated from the solution by sal ammoniac, it is merely necessary to add a solution of prussiate of mercury, for the

Prussiate of mercury added to the solution of crude platina, throws down the pure prussiate of palladium;

the precipitation of the palladium. Generally for a few ſeconds, and ſometimes for a few minutes, there will be no appearance of any precipitate; but in a ſhort time the whole ſolution becomes ſlightly turbid, and a flocculent precipitate is gradually formed, of a pale yellowiſh-white colour. This precipitate conſiſts wholly of pruſſiate of palladium, and when heated will be found to yield that metal in a pure ſtate, amounting to about four or five tenths *per cent.* upon the quantity of ore diſſolved.

Heat diſengages the pure metal which is not more than one two hund. part of the original ore.

The pruſſiate of mercury is peculiarly adapted to the precipitation of palladium, excluſive of all other metals, on account of the great affinity of mercury for the pruſſic acid, which in this caſe prevents the precipitation of iron or copper; but the proportion of mercury does not by any means influence the quantity of palladium, for I have in vain endeavoured, in the above experiment on crude platina, to obtain a larger quantity of palladium that I have ſtated by uſing more of the pruſſiate of mercury, or to procure any precipitate by the ſame means from a ſolution of pure platina.

More mercury does not augment the product.

The decompoſition is by double affinity.

The decompoſition of muriate of palladium by pruſſiate of mercury is not effected ſolely by the ſuperior affinity of mercury for the muriatic acid, but is aſſiſted alſo by the greater affinity of pruſſic acid for palladium; for I have found that pruſſiate of palladium may be formed by boiling a precipitated oxide of palladium in a ſolution of pruſſiate of mercury.

Pruſſiate of mercury is the teſt of palladium.

The pruſſiate of mercury is conſequently a teſt by which the preſence of palladium may be detected in any of its ſolutions; but it may be worth obſerving, that the precipitate obtained has not in all caſes the ſame properties. In general, this compound is affected by heat ſimilarly to other pruſſiates, but when the palladium has been diſſolved in nitrous acid and precipitated from a neutral ſolution by pruſſiate of mercury, the precipitate thus formed has the property of detonating when heated. The noiſe is ſimilar to that occaſioned by firing an equal quantity of gunpowder, and accordingly the exploſion is attended with no marks of violence unleſs occaſioned by cloſe confinement. The heat requiſite for this purpoſe is barely ſufficient to melt biſmuth, conſequently is about 500° of Fahrenheit. The light produced is proportionally feeble, and can only be ſeen in the abſence of all other light.

The precipitation from a nitrous ſolution detonates by low heat.

In

In endeavouring to dissolve a piece of palladium in strong colourless nitric acid for the purpose of forming the detonating prussiate, I found that, although the acid shortly acquired a red colour surrounding the metal, the action of the acid was extremely slow, and I was surprised to observe a fact that appears to me wholly singular; the metal was taken up without any extrication of nitrous gas; and this seemed to be the cause of the slow solution of this metal, as there was not that circulation of this fluid, which takes place in the solution of other metals until the acid is nearly saturated.

Palladium is very slowly acted on by nitric acid and extricates no gas.

As the want of production of gas appeared to retard the solution of palladium, I tried the effect of impregnating a quantity of the same acid previously with nitrous gas, and observed its action to be very considerably augmented, although the experiment was necessarily tried in the cold, because the gas would have been expelled by the application of heat.

Nitrous acid acts more strongly.

Beside those properties which are peculiar to palladium there are others, not less remarkable, which it possesses in common with platina. I have on a former occasion mentioned that these metals resemble each other in destroying the colour of a large quantity of gold. Their resemblance, however, in other properties is not less remarkable, more especially in the little power they possess of conducting heat, and in the small degree of expansion to which they are liable when heated.

For the purpose of making a comparison of the conducting power of different metals, I endeavoured to employ them in such a manner, that the same weight of each metal might expose the same extent of surface. With that view I selected pieces of silver, of copper, of palladium, and platina, which had been laminated so thin as to weigh each 10 grains to the square inch. Of these I cut slips $\frac{1}{10}$ of an inch in breadth, and four inches long; and having covered their surfaces with wax, I heated one extremity so as to be visibly red, and, observing the distance to which the wax was melted, I found that upon the silver it had melted as far as $3\frac{1}{4}$ inches: upon the copper $2\frac{1}{2}$ inches: but upon the palladium and upon the platina only one inch each: a difference sufficient to establish the peculiarity of these metals, although the conducting power cannot be said to be simply in proportion to those distances.

Conducting powers of palladium and platina as to heat, tried by the melting of wax upon them. The measure is not half those of silver and copper.

In order to form some estimate of the comparative rate of expansion of these metals, I rivetted together two thin plates of platina

Rate of expansion by heat, tried by rivetting bars together.

While steel expands through 12, platina will expand 9, and palladium 10.

platina and of palladium; and observing that the compound plate, when heated, became concave on the side of the platina, I ascertained that the expansion of palladium is in some degree the greater of the two. By a similar mode of comparison I found that palladium expands considerably less than steel by heat; so that if the expansion of platina between the temperatures of freezing and boiling water be estimated at 9 parts in 10,000, while that of steel is known to be about 12, the expansion of palladium will probably not be much more or less than 10, or one part in 1000 by the same difference of temperature.

It must, however, be acknowledged, that the method I have pursued is by no means sufficient for determining the precise quantity of expansion of any substance; but I have not been induced to bestow much time on such an inquiry, since the extreme scarcity of palladium precludes all chance of any practical utility to be derived from a more accurate investigation.

VI.

Report made to the Athénée des Arts of Paris, by MM. Rondelet, Beauvallet, *and* Duchesne; *on the founding the Statue of* Joan of Arc *in Bronze, by a Way never before used for large Works, by MM.* Rousseau *and* Genon, *under the Direction of M.* Gois, *Statuary.**

Casting in sand used hitherto only for small figures.

THE method of casting in sand hitherto has only been used for figures from 65 to 70 centimetres (about 2¼ feet) in heighth; while the statue, which was to be formed, being of much larger dimensions, should of course be managed according to the method called the *great foundery*, on account of its being used for colossal statues.

The great foundery known to the ancients, but was lost.

At its revival large statues were cast in separate parts.

This method of casting was known to the ancients, who were even superior to us in it; but this art was lost with many others, and in the time of the Medicis large statues were not formed at a single casting. The figures of Henry the Fourth and of Lewis the Thirteenth, which are seen at Paris, were

* Magazin Encyclopédique, T. I. p. 350.

placed

placed on horses made previously, one for the statue of Ferdinand, Grand Duke of Tuscany, and the other for that of Henry the Second, King of France.

The statue of Lewis the Fourteenth, in the Place de Vendome, is the first that was formed at a single casting since the revival of the art. It was suitable to so great a prince to permit his image to be made solely by a grand method; but Girarden and Keller, to whom the work was entrusted, then made their first attempts, which occasioned many faults, such as the casting it too thick, which in uselessly employing more metal, increased the difficulty of supporting the colossal figure; and such also as using unnecessary labour; but notwithstanding all their precautions, the casting did not succeed perfectly, and considerable repairs were obliged to be made in it.

That of Lewis XIV. the first cast after this in a single piece.

About the same time were erected the equestrian statues of this prince, at Boufflers and at Lyons, by the same Girarden, at Rennes by Coizevox, at Montpelier by Mazeline and Utrels, and at Dijon by Le Hongre.

Various other statues made in the same way.

After this, Le Moine had to found a statue of Lewis the Fifteenth at Bourdeaux, which met with great accidents; but he had more success with one at Rennes, which was a pedestrian statue: Guibal also made one for Nancy. But this art did not attain to a great perfection till Boucharden was employed to construct an equestrian statue of Lewis the Fifteenth at Paris: The great care of M. Goor prevented any accident from happening to it, or to that also which was cast at Reims by the same artist, from the model of Pigale. This founder had not the same success when he formed the statue of Frederic the Fifth at Copenhagen, from the model of Saly, which required great repairs. Finally, great improvements had been made in the art when the statue of Peter the Great was founded at Petersburg by Falconet, and nevertheless he was obliged to refound a second time the upper half of the statue.

The art not very perfect before the statue of Lewis XV. was cast.

Great improvements made in the art when the statue of Peter the Great was cast.

The great disadvantage of the method hitherto used, is its enormous expence and the great time it requires. It is true, that for works which are intended for duration economy is not the chief object; but if they can be performed equally well by M. Gois's method, at one-half the expence and in a fourth

Disadvantages of the great foundery.

M. Gois's method superior to it in various respects.

of

Reasons for believing that it would answer for the largest works.

of the time, it certainly ought to be preferred. There is good reason also to decide, that this method will do equally well for the largest works; for, according to calculation, the largest statue of this kind in France exceeds that of Joan of Arc by a much smaller proportion than the latter exceeds that of the largest statues ever before cast in the same manner, which never weighed more than from 8 to 10 kilogrammes (from about 17 to 22 lb.) The statue of Joan of Arc weighed 600 kilogrammes, which is 60 times more; but that of Lewis the Fifteenth, which weighed 17,000 kilogrammes, was only twenty-eight times heavier than that of Joan of Arc.

But in order to judge better of the advantages of M. Gois's method, it shall be described at large, and an account given also of the method of molding by wax, or of the *grand foundery*, in order to compare them together.

M. Gois's statue of Joan of Arc exhibited and admired.

M. Gois having made a statue of Joan of Arc for a prize, exhibited it in public in the year 10, (1802.) The prefect of the department of the Loire saw it, and proposed to the city of Orleans to re-erect that monument to the glory of this heroine, which had been destroyed in the anarchy of the revolution. It was accordingly ordered to be done. M. Gois being informed of this, went to Orleans and offered to make a cast in bronze from his statue, without precisely knowing whether it was that which the city required.

He is employed to make a cast in bronze from it for the city of Orleans.

An agreement was then made with M. Gois to complete the statue at a fixed price, in the course of about one year from the 5th Germinal, *An XI.* or before May 4, 1804.

Is induced to have the cast made in sand to save time and expence.

M. Rousseau undertakes the work for him, who had before made a fine cast of the Graces; and employs the same workman who cast them.

M. Gois began to be alarmed at the enormous expence of the usual method of casting such statues, and at the great time it required, which he feared would prevent his performing the agreement. He knew that M. Rousseau had made a cast from the groupe of Graces by Germain Pilon, with great success, by a different method; and though these figures were but 1,38 metres high, and his statue was more than two metres, ($6\frac{1}{2}$ feet) he went notwithstanding to consult this founder, who engaged for the success of the method, and promised to employ in the business the same workman who had cast the above groupe without having met with any accident: This last consideration determined M. Gois to entrust the work to the *founders in sand*.

The

The firft of Fructidor, *An XI.* the bufinefs was began; but as they commenced with the bas-reliefs, it was not till three months after that they undertook the work of the ftatue.

They made ufe of the common fand of the founders, which is argillaceous, and always kept a little moift. After having well raked it, feparated all the ftones, and broken all the lumps that could be met with, they filled with it a cafe of 2,20 metres long, and one metre broad at the infide, and 16 centimetres high; the thicknefs of the wood of the cafe was eight centimetres. The fand was ftrongly beaten with a rammer 10 centimetres broad and 60 long, and by this operation acquired fufficient confiftence to be raifed along with the cafe without any danger of running out. The procefs defcribed for cafting Joan of Arc in fand.

After this the ftatue was placed upon the firft cafe, which is called the *falfe mold*, becaufe it was to be afterwards replaced by another: the fand was ftirred up a little, to permit the moft prominent parts to enter it; another cafe of the fame fize was then put over the firft, and attached to it by four points of iron.

The true *concave mold* was then began, by modelling each part of the figure with the fame kind of fand. A workman of much addrefs and intelligence is requifite for the divifion and diftribution of the different pieces which form the mold: he fhould explain the motives which induced him to prefer one diftribution to another: each piece fhould have different fections: care fhould be taken to mold the parts which have a large and uniform furface in a fingle piece, while the pieces muft be multiplied for thofe portions of the ftatue which have many finuofities and deep indentations. Method of forming the mold.

This part of the operation requires the moft care; for if it be performed with negligence, the extraction of the model would be attended with great difficulties; and if the workman employed is aukward, numerous faults will need reparation after the cafting, and probably great accidents may happen. It is but juftice to fay, that Genon, the workman on this ftatue, fhewed in his performance equal dexterity and knowledge.

To prevent the pieces of the mold from adhering to each other, care is taken to powder the parts of each which is finifhed, with charcoal duft inclofed in a bag, before a new piece is began. The workman having finifhed the mold-

ings

ings of the contours of the figure, filled up the empty ſpaces between them and the caſe with ſand, which he firſt preſſed and forced together with his hands, afterwards beat it with the bat, and finally with a mallet; this compreſſion gives it ſuch a ſolidity that it appears like ſtone, or at leaſt like baked earth.

The ſame care was taken with each caſe as they were ſucceſſively added, to the number of ſeven; which compleating the top of the ſtatue, the whole was then reverſed in order to replace the loweſt caſe, which, as mentioned, was only a falſe mold; and then each part at the lower extremity was alſo modelled in ſame manner as the preceding.

The hollow mold being finiſhed, the caſes were taken aſunder, and each piece removed ſeparately to take out the ſtatue: then they were all placed in their proper order in the exterior mold, which may be compared to the cover uſed by thoſe who make plaſter-of-Paris caſts. Each piece would be well retained in its place by its irregular form; but it was ſtill farther faſtened by a little thin paſte made of flour, which was applied by a bruſh both to the pieces themſelves and the parts that adhered to the caſes. It was thought neceſſary to take a precaution more than what was uſual, through the apprehenſion that the paſte would not hold together thoſe large pieces as well as it did the ſmall pieces in leſſer works; they were therefore traverſed by long wires of iron, which entered into the cover or exterior mold.

The pieces of the mold cemented together with paſte, and ſecured by wires paſſing into the caſes.

This mold being thus entirely compleated, had only to be dried till the time of the caſting. A new mold was neceſſary to be made to caſt the core: the ſame pains were not taken with this as with the firſt, as it would be uſeleſs to do ſo. When this ſecond mold was finiſhed, a coat of modelling-clay was applied to its inſide, of the ſame thickneſs which was intended to be given to the bronze; and without waiting for its drying, it was cloſed and the core caſt in it, which was compoſed, as is uſual, of equal parts of plaſter of Paris and of brick duſt.

Method of forming the core.

Eight rods of iron having been placed at the inſide of the mold, afterwards projected from the core about 10 or 12 centimetres, which ſerved to place it with preciſion in the hollow mold.

Eight iron rods laid ſo as to project a few inches from the core, in order to place it in the mold.

But

But in the mean time the cafes had been placed one over the other, and the iron pins which connected them fitted to their places, taking care to divide them into two portions, which anfwered to the two cafes ufually employed by the founders; and which, inftead of the ufual thicknefs of five centimetres at moft, were, the one 48 and the other 64 centimetres thick.

In this ftate they were dried, by placing them round a brafier of kindled charcoal, the fire of which had the more power from the mold being divided into two portions, and empty. The pieces of the mold dried.

The core was likewife dried by placing it over a brafier of charcoal; the fame was alfo placed round it; and in eight hours the moifture was entirely evaporated. It was left to cool, and it was placed in one half of the mold; the fecond half of the mold was afterwards fitted on, and the whole compreffed together by iron preffes in the ufual manner. The core alfo dried. The mold and core put together ready for cafting, and fecured by preffes.

After this there only remained to conftruct the bafon, *(l'echino)*, to fufe the metal, and make the caft. Thefe operations being the fame for both methods of founding, fhall be related, after firft as briefly as poffible defcribing the method of cafting by the great foundery in which wax is ufed.

The firft operation for the great foundery is to dig a trench proportionate to the fize of the figure to be caft, and to furround it with a wall to prevent the earth from tumbling in. After the model is finifhed it is oiled, and a mold formed from it with plafter of Paris in the ufual way, and with the precautions before directed for molding in fand: In each piece of this mold rods of iron are inferted, by which they may be eafily lifted when the mold is taken afunder or put up; each of thefe pieces is numbered, that its proper place may be known. Defcription of cafting by the great foundery. A mold formed from the ftatue in feparate pieces.

After this feveral layers are applied with a brufh to the infide of the pieces, of a compofition made of 7-tenths of yellow wax, 1-tenth of turpentine, 1-tenth of white pitch, and 1-tenth of hog's-lard, which is melted flowly to prevent its forming bubbles. A compofition of wax laid on to the infide of the mold, of the thicknefs intended for the bronze.

When the different layers form a thicknefs of three or four millimetres, (0.15 inch) cakes of wax are placed infide in thofe parts where the bronze fhould be of a greater thicknefs, and faftenings of fheet-brafs are inferted, which may take hold of the core and prevent the wax from falling off.

The

The core cast in this mold, and the modelling of the wax finished by the statuary after it is taken out.

The mold is then fitted together, and the core cast with quickness, that it may form an entire mass, and not lie in layers: As soon as it is solid the model is taken asunder, and the statuary repairs the wax, takes off all the sutures of the molds, rectifies the errors which may have occurred, and gives to the work all the perfection of which it is susceptible.

The true mold formed over the wax; its composition

After this the true mold is formed of materials capable of supporting the heat and the impulse of the metal; to compose which three-sixths of earth are mixed with one-sixth of horse-dung, and left to rot in a ditch for one winter: when this mixture is taken out, two-sixths of broken crucibles, well pounded and passed through a sieve, are added: the whole is tempered with urine and beat up on a stone: it is then what is called *potee*.

Formation of the true mold continued.

When it is to be used, a sufficient quantity is taken and water enough added to it, to make it capable of being laid on with a brush; forty coats of it are then laid over the wax successively, (care being taken that one coat is dry before another is laid on), which altogether form a thickness of five centimetres (2 inches.) The mold is then surrounded with flat bands of iron, which cross each other like net-work; then, after rendering the *potee* thicker, by adding earth to it, and hair that has been well beaten, it is laid on over the former work with the fingers, until the mold has attained the thickness of twenty centimetres below and sixteen above (6 and 8 inches); after which it is surrounded a second time with bands of flat iron.

Preparations for, and method of melting out the wax.

After this a wall is built round it, the passages necessary for the fire constructed, and the intervening spaces are filled up with broken bricks: Then the fire is kindled in the passages most distant from the figure, and is gradually increased for nine days, and again diminished for the same space of time. On the second day the wax begins to flow, and continues to do so for ten or twelve days; about half of it is lost.

When the fire is extinguished, the work is left some days to cool; then the broken bricks are removed, and before the mold is buried, a coat of plaster, about half an inch thick, is put over it, which is called the *chemise*. Then they proceed to bury the mold, or inclose it with earth, first taking care to stop all the ways through which the wax flowed, and to raise the

The mold buried, and the pipes and vents placed ready for casting.

the pipes for the vents and for the entrance of the fused metal. The earth used for enclosing the mold should be first skreened, and then laid on equally in the excavation. After each course is raised to a thickness of thirty centimetres, (1 foot) it is beaten down till it is reduced to ten.

After this there only remains to build the bason for the reception of the metal, called the *echino*.

In enumerating the operations necessary for the method of casting in sand first mentioned, the authors of the report state them to amount to ten; while those used for casting in the large way last recited, in which wax is used, amount to no less than twenty-eight, each of which they particularize; but as these operations may readily be counted from the relation already given, this catalogue is not inserted here. They also remark, that the laying on the wax on the pieces of the mold takes up much time, as does likewise the preparation of the *potee*: that in the first method the circling with bands of iron is entirely avoided, and the building of the passages for the fire, which are very expensive: that likewise the molding and setting up of the vast number of pipes and vents is saved in it; and in the drying of the work the economising of fuel is greatly in favour of the first method, for in it these operations are performed in a short time with a very small fire, which in the other method require at least three weeks and a powerful heat: that in the repairing of the wax the statuary must work with his own hands: and that in taking out the statue when cast, there is vastly less trouble in the first method.

Enumeration of the various advantages of casting in sand, and disadvantages of the great foundery.

The authors here describe the method of erecting the furnace for fusing the metal for the statue of Joan of Arc; but as it was constructed to burn wood, which fuel is not used in our founderies, and as the description would be on other accounts of but little benefit to our artists, it is omitted.

It is only necessary to state, that the place which contained the fused metal was at such an elevation, that, when the stopper which retained it was driven in, it might flow freely into the *echino* through the passage prepared for it.

The mold for the statue was partly buried in the earth, so as to allow a fall for the metal of eight centimetres (3 in.) from the hearth to the entrance of the pipes; and the authors observe, that the trouble of burying the mold might be avoided by laying

The mold of the statue of Joan of Arc laid in the earth ready for casting.

ing it on its side, for which position they think it was sufficiently well prepared.

Parts of the statue cast separate from the rest.

The statue was all formed in one mold, except the skirts, *(pleinte)*, one arm, and the plumes of the helmet, which were placed in a separate case: This might have been dispensed with, but it was apprehended that, if they remained with the statue, they would have much encreased the difficulty of the work, by adding to the elevation of the figure.

1000 kilograms of metal fused.

Every thing being prepared for the casting, about 1000 kilograms (about 32 C.) of the metal was placed in the furnace, one half of which metal consisted of old cannon, a third of copper, and the rest of brass; and on the 8th of Germinal, *An XII.* (29th March, 1804), at eight o'clock in the afternoon, the metal, after five hours heating, being in compleat fusion, and the *echino* and the stoppers which closed the two passages for the metal being previously heated, M. Rousseau forced in the plug that retained the metal in the furnace; it flowed immediately into the *echino*; the stoppers were removed from the passages for the metal from thence, and in less than two minutes a little of the metal appeared at the vents, and shewed that the cast was completed.

The cast made by M. Rousseau: It succeeded compleatly.

The statue requires no repairs or additions.

On removing the sand it was found that no accident had happened but a slight flaw on the stomach of the figure; that the head was quite perfect; and that there had been no partial casting, or any part of the figure necessary to replace, which often happens in the other method.

The reporters recommend the *Athénée des Arts* to give medals to MM. Rousseau and Genon, and to make honourable mention of M. Gois.

The reporters conclude with high encomiums on the advantages of this method of casting, and recommend that medals be given to MM. Rousseau and Genon by the Athénée des Arts, in testimony of their merit; and that as the rules of the Society prevented this recompence from being granted to any of its members, honourable mention should be made of M. Gois.

Experiments

VII.

Experiments made at the Galvanic Society of Paris, by M. RIFFANT, *Director of the Nitre and Gunpowder Works, tending to prove that Muriatic Acid is not composed as announced by M. Pachiani* *.

AS soon as the Galvanic Society knew that M. Pachiani, of Pisa, had announced, that he had obtained muriatic acid by depriving water of a portion of its oxigen, their first care was to engage in a course of experiments both by galvanism and electricity, to obtain, if it was possible, a confirmation of a discovery so important to the progress of science. The society had a letter communicated to them, which was addressed on the 9th of May, 1805, by M. Pachiani, to M. Pignotti; in which he recited the results which he had obtained, but without entering into any detail relative to the nature or order of his experiments; they only knew that he used the galvanic pile. They therefore determined to make their experiments with the same apparatus, in the manner which appeared to them the least likely to produce results liable to objections. Two of these experiments, which appeared principally worthy of attention, were conducted as follows: The galvanic Society engage in experiments to determine the truth of Pachiani's position.

Experiment I.

A portion of a new glass tube was taken, three inches long, and 0.35 inch in diameter inside, one of the ends of this tube was closed at the lamp; to the other end was united a capillary tube bent in such a manner as to pass under a jar, and at equal distances from the junction of the capillary tube, two points were drawn out at the lamp in the thickness of the glass, by means of which two bits of gold wire of about 0.02 inch in diameter, and of the standard 0.976 of purity, were inserted in the tube, at a small distance from its lower extremity, and disposed so as not to touch each other, or bear against the sides of the tube; these points of the glass were then closed at the lamp. The tube and its capillary prolongation were filled with distilled water, whose Description of the apparatus used in Exp. 1st,

* *Journal de Physique*, Tom. LXI. p. 281.

purity

purity had been proved by nitrate of filver; the whole was faftened by wax on a fmall piece of glafs, placed in the midft of an horizontal galvanic pile of fifty-two double fquare plates of 4.25 inches at each fide. Thefe plates were feparated by pieces of leather, which formed between each other divifions, which were filled with very pure fand, moiftened by a folution of muriate of foda. The capillary tube was paffed beneath the water of a ciftern, with its extremity under the mouth of a jar filled with water. The two wires of gold being made to communicate with the two poles of the pile, its activity was immediately exhibited by the difengagement of gas in chains of bubbles very apparent, parting from the extremity of each of the gold wires, but in a more confiderable quantity from that correfponding with the copper pole. This activity of difengagement of gas continued without interruption from the eighth of Thermidor, to the 23d of the fame month, on which day, after the pile being moiftened with folution of muriate of foda, it ftopped for fome time: it foon however recommenced, and always did fo after any fufpenfion. Its activity was immediately renewed by agitating the wires which communicated with the poles of the pile. The activity of the pile was conftantly greateft at four o'clock in the afternoon; and immediately afterwards it began to diminifh. On the 11th of Fructidor the apparatus was taken afunder, after continuing for thirty-four fucceffive days in continual action. The water was then diminifhed to half its original volume, but had not loft any of its limpidity.

The fand of the pile moiftened with folution of muriate of foda: gas immediately difengaged on its completion.

The activity of the difengagement of gas renewed by agitating the wires. This activity always greateft at four in the afternoon. The apparatus feparated after continuing in action thirty-four days.

The extremities of the gold wires, from whence the gas had proceeded, were oxided, but that moft perceptibly which communicated with the zinc pole of the pile. The whole gas obtained and collected during the experiment, was 793 cubic centimetres (1200 inches). The liquor remaining in the tube was carefully examined; it produced no fenfation of tafte on the tongue, had no action whatfoever on the tincture of turnfol and of fernambuc, and did not produce the leaft cloudinefs in the folution of nitrate of filver.

The water in the tube diminifhed one half, the gold wires oxided, that next the zinc moft, 793 cubic centimeters of gas collected. The remaining liquor in the tube has no tafte, has no action on turnfole, and is not affected by folution of nitrate of filver.

The gas produced by the action of the pile was then examined. After having introduced one meafure of it into the eudiometer of Fontana, an equal meafure of nitrous gas, made purpofely for the proof, was added; an abforption of feventy-

feventy-feven two hundredth parts took place in the volume of the two meafures.

The gas produced is tried by Fontanas's eudiometer, an abforption of feventy-feven two-hundredths takes place.

In order to afcertain whether all the oxigen which the gas contained had entered into combination in this abforption, a fecond meafure of nitrous gas was introduced into the eudiometer; but the gas did not experience any diminution of its volume. The quantity of oxigen which the abforption produced by the introduction of the firft meafure of nitrous gas might indicate, was attempted to be valued by a comparifon with atmofpheric air effayed in the fame manner; for which purpofe one meafure of atmofpheric air was introduced, and an equal quantity of nitrous gas added; an abforption of fifty-five two hundredth parts took place in the volume of the two meafures. In confidering this abforption as the effect of the combination of nitrous gas with the volume of oxigen gas, correfponding to the 0,22 parts, which atmofpheric air contains, it may be concluded, that the abforption of feventy-feven two hundredth parts, produced with the gas of the pile, reprefented proportionally the combination of the fame quantity of nitrous gas with a little lefs than 0,31 parts of the oxigen. It was then obferved that the meafures of gas having been introduced feparately and fucceffively into the eudiometer, it might happen that they were not fufficiently well mixed, and that confequently the abforption was not as compleat as it might be. It was thought that it might be better to pafs the gafes in feparate meafures under a jar, and then to introduce the whole volume together into the eudiometer. The former experiments having been repeated in this manner, an abforption took place between the gas of the pile and the nitrous gas, of ninety-two two hundredth parts in the volume of the two gafes, in place of feventy-feven refulting from the fame proof, by the firft method; and with the atmofpheric air and the nitrous gas the abforption was fixty-eight two hundredth parts inftead of fifty-five. There refults then from this, according to the fame ratio of the 0,22 parts of oxigen contained in the atmofpheric air, a proportional indication of about 0,30 parts of this gas contained in that of the pile. It was again proved with the eudiometer of Volta, by introducing a fingle meafure into it, through which the electric fpark was made to pafs; the proof was afterwards repeated fucceffively.

The quantity of oxigen contained in the gas valued at 0,31 parts.

The proportion of the oxigen after more exact trials is more accurately valued at 0,30 of the gas.

The gas farther proved by Volta's eudiometer gives the same result of 0,30 oxigen.

successively, on two, three, and four measures, and always the absorption resulting from the inflammation by the electric spark, gave the same indication of about 0,30 parts of oxigen.

Experiment II.

Apparatus used in second experiment described. The sand is moistened with river water containing 1-fixtieth nitric acid. The gold wire is dissolved and precipitated,

Two grammes (31 gr.) of distilled water were put into a glass tube bent into the form of a syphon; two wires of gold of commerce of about 0,008 inch in diameter, passing into the water at about 0,024 inch distance from each other, were inclosed in this tube; the tube was then placed upon an horizontal pile of fifty double plates, of about $3\frac{1}{4}$ inches in each side. The intervals between them were filled up with dry sand, and then moistened with river water acidulated with about $\frac{1}{60}$ of nitric acid. The wires of gold having been placed in communication with the two poles of the pile, the water in the tube assumed in the first day a reddish brown tint at the side of the copper pole, and the wire which passed to that part was covered with a coat of oxide of gold of a deep brown colour. The wire which communicated with the zinc pole did not assume the same tint; the gold of the wire was dissolved by degrees, and was precipitated together with a part of the silver. This precipitate exhibited with a magnifying lens, over almost the whole length of the tube, crystals in needles. The wire corresponding to the zinc pole was entirely deprived of the gold which covered it, and then only consisted of a thread of silver of extreme tenuity. But very little gas was discharged from either extremities of the wires. The water was not diminished a fiftieth part of its volume.

very little gas produced, the water in the tube is not diminished 1-fiftieth.

The pile continued in action forty days. Indicates by the electrometer an intensity of 840°. The remaining liquor in the tube shews no trace of acidity by any of the reagents, and has a metallic taste.

The pile continued in activity 40 days from the 28th of Messidor to the 8th of Fructidor. It indicated then on the last day by the electrometer (simplified by one of the Members of the Galvanic Society, from that constructed in Germany, described in the *Journal de Physique* for the month Messidor, an. 13,) an intensity of 840 degrees. The liquor remaining did not afford, by any of the different reagents, the least trace of oxidity; a metallic taste was alone perceptible in it.

The galvanic society, in examining chiefly the results of the first experiment, as corresponding more particularly with the fact announced by M. Pachiani, have considered, that in allowing

allowing for the small quantity of oxigen which had caused the oxidation of the extremities of the gold wires, the whole quantity of the oxigen contained by the gas of the pile may be valued in a very near approximation, at 0,31 of its volume; and it is very nearly in this same proportion that oxigen gas enters into the formation of water; it was thought that it might thence be concluded, that the whole effect of the galvanic pile, during the entire continuation of the experiment, had been the decomposition of a part of the water employed, and the separation, in their state of purity, of the oxigen and hidrogen gases which composed it. The Society then are of opinion, that M. Pachiani was deceived as to the nature of the acid which he announced that he obtained, and that this acid * might have been produced by some animal or vegetable substance employed in the apparatus. The Society does not hesitate to declare, that with the apparatus which they used in preference, (as being the most simple, and the least liable to the influence of other matters,) they do not think it possible to effect any thing by the action of the galvanic pile, but a decomposition more or less great, of the water used in the experiment.

The Society think the action of their pile only decomposed the water into oxigen and hydrogen gases, and that Pachiani is mistaken, and that the acid found in his experiment was produced by other means than those he announced, and that it is not possible to effect any thig by the galvanic pile in water but its decomposition.

VIII.

Account of an Ancient Geographical Tablet in the Museum of Cardinal Borgia, from † a Memoir presented to the Academy of Gottingen, by Professor Heeven.

In the Museum of Cardinal Borgia there is deposited an ancient geographical tablet, from which an engraving has been

The tablet was found in the museum of Cardinal Borgia;

* M. Giobert, in Van Mon's Journal, pretends, that the acids and salts used in the pile *circulate along the wires*, and pass into any liquor into which they are conducted; which does not appear probable.—B.

The galvanic apparatus used in these experiments is called a pile through the whole of the French paper, though from its horizontal position the appellation does not seem very proper. *Trans.*

† This memoir is entitled, "explicatio planiglobii orbis terrarum faciem exhibens, ante medium sec. xv. summa arte confecti; agitantur simul de historia mapparum geographicarum recte instruenda consilia.

The deſign is two feet in diameter, and done in coloured enamel,

limits of the countries not marked, accounts inſerted in it of various remarkable things.

been made, one of the impreſſions of which is in the poſſeſſion of the author of the Memoir. This remarkable monument is not a chart drawn by the pen, but a round tablet, of which the deſign occupies a ſpace about two feet in diameter, on which the hemiſphere, known at that time, is repreſented in coloured enamel, like a round ſurface. The countries and the places are marked by their proper names, but the limits of the countries are not traced; the mountains, the rivers, the people, and all the things remarkable (as the animals, the battles, the caravans, the bazars, the camps, the wandering tribes, &c.) are repreſented and explained on it by inſcriptions in the latin tongue, but written in German characters. It may be conceived from this firſt view how intereſting this monument is, and alſo with what art it is executed, ſo that it is impoſſible to ſuppoſe, that it was made for the uſe of a private perſon. Its date is not mentioned, but it may be determined with certainty that it was conſtructed in the firſt half of the fifteenth century. In reality, the moſt recent event marked on it is the victory of Tamerlane over Bajazet in 1402; there is no mention of the taking of Conſtantinople, or the leaſt trace of any of the diſcoveries of the Portugueſe. Of the geographical charts known at preſent, that of *Marino Sanudo* at the commencement of the fourteenth century is the only one certainly more ancient; but that of *Andre Bianco* of 1436, which *Formaleoni* has made known, is very nearly of the ſame time as this monument. No general ſource of information can be diſcovered by which the author of the tablet has been aſſiſted. It is not made according to documents from Ptolemy; it more follows thoſe of the Arabians, eſpecially with regard to Africa; of the names which are found in the works of *Marco Paolo,* and the other more ancient travellers in Aſia, only ſome are ſeen on that part of the world. The extent of Europe is repreſented as much greater than that of Africa, and at leaſt as large as that of Aſia. The following are ſome of the moſt remarkable particulars of it: Sweden is ſet down under the name of *Magna Gothia,* and Denmark is wanting. In Pruſſia, the ſeat of the wars of the Teutonic order with the Lithuanians, repreſented with this inſcription: *Hic ſunt confinia paganorum et chriſtianorum, qui in Pruſia adinvicem continuo bellant.* It may be perceived by this, that the Lithuanians were therein conſidered

Why ſuppoſed to have been made in the firſt half of the fifteenth century.

It is the oldeſt geographical deſign, except the chart of Sanudo.

Sweden called Magna Gothia in it, Denmark omitted, Lithuanians called Pagans,

ſidered as Pagans, although chriſtianity was introduced among them before this period. Ruſſia appears under the denomination of Tartary, and near the Caſpian Sea and the ſea of Aſof, are repreſented the famous Bazars of thoſe times. England and Scotland appear at the border, but there is no more room for Ireland. Africa exhibits none of the diſcoveries of the Portugueſe, but the northern half of it was known to the author as far as Soudan. He names not only the villages along the coaſt, but he moreover knows that the inhabitants of mount Atlas, the people of Barbary are at war with the Saracens. Near theſe mountains is inſcribed, *In illis montanis habitant plures principes et reges, et habitant continuo in tentoriis, et præliantur continuo contra Saracenos, et contra juxta caſtra et civitates* *. In Egypt the junction of the grand caravan of Mecca is marked, and not only the names of the deſerts of ſand are inſerted, but thoſe alſo of the places moſt important to commerce, as Tagaza, Ganufia, &c.

Ruſſia called Tartary in it. Places of the Bazors repreſented.

It contains none of the diſcoveries of the Portugueſe in Africa.

The junction of the Caravans of Mecca noted in Egypt.

The kingdom of Preſter John extends in Nubia *ab oſtio gandis* (Cape Gardefan) *uſque ad fluvium auri*. Bianco likewiſe ſets down the kingdom of Preſter John in Africa in the ſame manner, ſo that the Portugueſe are not the firſt who have thus deſcribed it. Aſia does not preſent fewer ſingularities. In Aſia Minor the camps of the Tartars are repreſented; *Tartaria reges maxima, qua Tartari cum ſuis jumentis et bobus excurrunt, civitatem ex multis tentoriis et carutes ſituant.* India is divided into *India ſuperior*, where the body of St. Thomas is found, and many chriſtian kingdoms, and *India interior, in qua Cathai civitas et magnificanis Imperatoris Tartarorum ſedes.* China is likewiſe inſerted in it, and its capital Cambalck (Cambalu Pekin) is alſo named. On the frontiers of little Bucharia at Organti, (Urgang)

Extent of Preſter John's kingdom ſet down.

Camps of the Tartars in Aſia Minor marked.

the diviſions of India, and poſition of Cathai inſerted, and China with its capital Cambalk (Pekin.)

* The Latin of the inſcriptions in this paper is not very correct, *continuo* is uſed in them for *ſemper*, and *juxta* for *vicina*, the word *ſituant* is alſo improper, and ſome others, but theſe circumſtances perhaps only mark more ſtrongly the authenticity of the account. In the inſcription relative to India interior, a ſmall alteration has been made from the memoir in this tranſlation. The word *magnificanis* has been formed from *magni canis* in the memoir, which being ſo printed, evidently was an error.—B.

The route of the caravans to Cathai; the country of Gog and Magog; and the site of Paradise.

de Organti ad Carthagium vadunt cameli in quatuor menfibus, the caravans going and returning to Cathai are reprefented. On the eaftern border the country of *Gog* and *Magog* is fet down, and finally *locus deliciarum* or paradife.

IX.

Analyfis of Birdlime. By M. BUILLON LAGRANGE.*

SECT. I.

The Origin and Preparation of Birdlime.

Various opinions on the nature of birdlime;

THE fubftance known by the name and appellation of *birdlime*, has been claffed among the immediate productions of vegetables.

M. Fourcroy,

M. Fourcroy was the firft perfon who confidered this matter to be glutinous: he has defcribed it as a fpecies in his "*Syfteme des Connoiffances Chimiques*," Vol. VIII. p. 306.

Birdlime, according to this chemift, may be made of the berries of miftletoe, or of the tender bark of the holly, and feveral other kinds of trees, macerated in water. Although this fubftance appears to have been hitherto not examined with fufficient accuracy, many qualities have been difcovered in it analogous to thofe of gluten,

Excepting a few chemical properties, mentioned in my "*Manuel d'un Cours de Chimie*," third edition, I have never found in any work the leaft elucidation of the nature of this fingular fubftance.

M. Chaptal,

M. Chaptal, in his "*Elemens de Chimie*," fpeaks only of its preparation. As the method prefcribed by this chemift is nothing different from that in the "*Materia Medica*" of

Geoffroy, Valmont de Bomare.

Geoffroy, and in the "*Dictionary*" of Valmont de Bomare, I fhall quote the article itfelf.

Ancient mode of preparing it, by boiling and pounding the berries of miftletoe;

"The ancients made ufe of the berries of the miftletoe of oak in the preparation of birdlime. The berries being firft boiled in water, were pounded, and the hot water was then poured off, in order to carry away the feeds and rhind. At prefent birdlime made of the bark of holly is preferred. The middle bark is made choice of, as being the moft tender and

* Annales de Chimie, Vol. LVI.

green:

green: this is placed in a pit to rot, after which it is pounded in mortars until it becomes a paste, and is then washed and cleansed with water. This substance has been considered as discussive and emollient, when applied outwardly."

At present it is made from holly bark rotted and pounded.

It is already known that the mistletoe of oak is employed in several pharmaceutical preparations; as, the universal water, the antispasmodic powder, Guttet's powder, &c.

Mistletoe of oak used in pharmacy.

In England, according to Geoffroy, birdlime is made of the bark of holly. He says, the bark is boiled in water seven or eight hours, till it becomes soft and tender. This is laid in masses in the earth, and covered with stones, placing one layer over another—the water having been previously drained from the bark. In this state it is left to ferment and rot, during a fortnight or three weeks, in which time it changes to a kind of mucilage. It is then taken from the pit, pounded in mortars till reduced to a paste, washed in river water, and kneaded till freed from all extraneous matters. The paste is left in earthen vessels during four or five days, to ferment and purify itself. It is afterwards put into proper vessels, and thus becomes an article of commerce.

English mode of making birdlime: Bark of holly is boiled, rotted, pounded, washed, and kneaded.

This mode of preparation is not universally followed, as every county has its peculiar way: there are even those who make a secret of the process.

At Nogent-le-Rotrou birdlime is manufactured by cutting in small pieces the second bark of the holly, fermenting them in a cool place during a fortnight, and then boiling them in water, which is afterwards evaporated.

Method pursued at Nogent-le-Rotrou; first rotting and then boiling:

At Commerci and its environs birdlime is obtained from several shrubs, as the holly, the wild vine (*viburnum lantana*, Lin.) and the mistletoe of every species.

At Commerci.

The best is that made from the prickly holly, which is greenish; that obtained from the *viburnum lantana* is of a yellowish tint. In using this latter, the epidermus is rejected, and only the second bark employed.

Best sort from the prickly holly.

The birdlime which I used in my experiments was made from the second bark of holly; and on comparing it carefully with some which had been sent me from Commerci, I found there was no apparent difference between them. I thought this precaution essential to obtaining greater precision in my analysis. It is well known that the birdlime of commerce is never in a pure state; it is frequently a composition of vegetable

Birdlime of commerce frequently adulterated.

table and animal matter; ſometimes it is even adulterated with turpentine, oil, vinegar, &c. It was therefore neceſſary that I ſhould be certain as to its purity; and by the following mode, which I adopted, I obtained birdlime of the very beſt quality.

The author's preparation of birdlime.

Having procured a ſufficient quantity of the ſecond bark of holly, I bruiſed it well, and boiled it in water for four or five hours: the water being poured off, I depoſited the bark in pits, in earthen pans, where it remained till rotten, or at leaſt till it became viſcous, moiſtening it from time to time with a little water. When it had obtained a proper degree of fermentation, it was cleanſed, by waſhing, from all heterogeneous matters.

Sect. II.

Chemical and phyſical Characteriſtics of Birdlime.

Characteriſtics.

Birdlime is of a greeniſh colour, and of a ſour flavour: it is gluey, ſtringy, and tenacious. Its ſmell reſembles that of linſeed oil.

It becomes dry and brittle by expoſure to the air;

Spread on a glaſs plate, and expoſed for ſome time to the action of air and light, it dries, and becomes brown in colour, being no longer viſcous. When quite dry, it may be reduced to powder, in which ſtate it is totally diveſted of its

but viſcid again when wetted.

adheſive qualities, and only recovers them on the addition of water.

Birdlime reddens tincture of turnſole.

By moderate heat it fuſes.

When gently heated in a porcelain veſſel it melts, but does not become very liquid; it ſwells in bubbles, which float upon the ſurface. This kind of fuſion produces ſmall black grains, which render the birdlime grumous: it produces a ſmell very ſimilar to that obtained from animal oils, on raiſing their temperature.

If this fuſion be continued for ſome time, the birdlime aſſumes a browniſh colour; but recovers its proper characteriſtics on cooling.

Strong heat inflames it;

Placed on red-hot coals, it burns with a briſk flame, and creates a great deal of ſmoke.

Heated in a crucible of platina, it takes fire when the crucible is red-hot; produces a lively flame, which riſes to about two decimetres, accompanied with a conſiderable quantity of ſmoke,

ſmoke, which eaſily attaches to the chimney: this combuſtion always takes place, although the crucible be taken out of the fire.

A whitiſh reſidue is left, which is very alkaline and partly ſoluble in water. Re-agents demonſtrate the preſence of ſulphate and muriate of potaſh. leaving an alkaline reſidue.

That part which water could not take up, on being put into muriatic acid was diſſolved with ſome efferveſcence. Experiments on the reſidue.

This liquid is copiouſly precipitated by the oxalate of ammonia; pruſſiate of potaſh gives a blue precipitate. That produced by ammonia is of a paſty conſiſtence, partly ſoluble in cauſtic potaſh: whence may be inferred that the reſidue, independent of the ſalts ſoluble by water, is compoſed of the carbonates of lime and of alumine, with a ſmall portion of iron. It contains ſoluble ſalt and carbonate of lime, and alumine and iron,

Water has very little influence on birdlime. On boiling, the matter does not completely diſſolve, but acquires merely a ſmall increaſe of fluidity, which it loſes in cooling, and reſumes its primitive conſiſtency. Water has little action on birdlime:

This water obtains no colour; its flavour is at firſt inſipid, afterwards ſour, and it reddens tincture of turnſole.

Evaporated to the conſiſtency of a ſyrup, it becomes coloured, with a mucilaginous appearance, which may be ſeparated by alcohol.

The action of water, therefore, is confined to the ſolution of a mucilaginous ſubſtance, with a ſmall portion of the extractive matter. It takes up ſome mucilage.

It is not thus with cauſtic potaſh. Its concentrated ſolution forms at once with birdlime a whitiſh magma, which turns brown on evaporation, with a ſeparation of ammonia. Cauſtic potaſh combines with birdlime and gives out ammonia.

This compoſition is leſs viſcid; it acquires a great degree of hardneſs by expoſure to the air; and its ſmell and taſte are ſimilar to thoſe of ſoap.

It is chiefly ſoluble in water and alcohol, there remaining but a few vegetable dregs. Theſe ſolutions are affected by ſtrong acids; but theſe kinds of decompoſitions preſent no new phenomena to thoſe obtained with a ſolution of ſoap. The compound has the habitudes of ſoap.

The moſt feeble acids ſoften birdlime, and partly diſſolve it: when concentrated, they act in a different manner. Weak acids ſoften and partly diſſolve birdlime.

Sulphuric acid renders it black and charry: by adding powdered lime, ſo as to form a thick magma, a ſeparation of ace- Sulphuric acid charrs it.

tic acid and ammonia is procured. There can be no doubt but that in addition to the acetic acid naturally present in birdlime, more is produced by the action of the sulphuric acid.

Hot nitric acid decomposes it, and forms a kind of wax:

Nitric acid, whilst cold, has very little power over birdlime: but on increasing its temperature, the mixture turns yellow, dissolves, and as evaporation advances swells considerably, leaving at last a hard brittle mass. This mass, when a second time submitted to the action of nitric acid, is dissolved, a part being converted into malic and oxalic acids. By continuing the evaporation a yellow matter is obtained, easily friable, yielding to the pressure of the fingers like wax, with a kind of elasticity, and melting by means of a gentle heat.

which forms soap with potash.

Potash combines with this matter, changes its colour from yellow to brown, and forms perfect soap.

Alcohol partially dissolves it.

Alcohol partially dissolves it, and becomes yellowish; its transparency is diminished by the addition of water.

On evaporating the alcohol to dryness, there remains a yellow matter divested of the greasy appearance, which yields a sweet odour in burning.

Cold muriatic acid has little action on birdlime.

Cold muriatic acid has no action upon birdlime: when heated it turns it black.

Oxigenated m. acid alters birdlime considerably.

Oxigenated muriatic acid operates differently. Either by mixing the gas with the water containing the liquid birdlime, or by shaking it in a bottle with the acid in a very concentrated state, the following phenomena were equally observed;

The birdlime quickly lost its colour, and became white; it was no longer viscid, but divided into hard compact portions, containing in their centre a quantity of birdlime, which the oxigenating principle had not reached. This non-oxigenation may be attributed to the difficulty there is of preserving this substance in its liquefied state in hot water, whereby the operation of the acid is confined to its exterior surface.

Characteristics of oxigenated birdlime.

The characteristics of oxigenated birdlime are—

1. It is capable of being reduced to powder.

2. It is insoluble in water, even when heated.

3. It does not become liquefied at a high temperature.

4. It will not turn yellow, nor will it form a resin with nitric acid.

Acetous

Acetous acid softens birdlime and dissolves a certain quantity; the liquor acquires a yellow colour; its taste is insipid. Acetous acid softens birdlime, &c.

Carbonate of potash produces no precipitate; evaporation gives a resinous residuum, which cannot, however, be brought to a state of perfect dryness.

Certain metallic oxides are easily reduced on being heated with birdlime. Birdlime reduces metallic oxides.

Semi-vitreous oxide of lead assumes a grey colour, dissolves, and forms with the birdlime an emplastic mass. Semi vitreous oxide of lead incorporates with birdlime.

Alcohol at 40 degrees, and boiling, dissolves birdlime so long as it is kept hot; it is clear, and of a transparent yellow colour; but in proportion as the liquor cools, it becomes turbid. Boiling alcohol dissolves birdlime,

A yellow matter may be separated by filtering, which is much softer than the original mass; melts in a moderate heat, diffusing a smell analogous to that of wax, of which it seems to possess all the qualities. and lets fall a kind of wax by cooling;

The filtered liquor is bitter, nauseous, and acid; precipitating in water, and leaving on evaporation a substance similar to resin. retaining a resin.

Sulphuric ether may be considered as the true solvent of birdlime: its action on this substance is rapid, first dividing it, and then dissolving it nearly *in toto*, there remaining only a few vegetable dregs. The liquor acquires a greenish yellow colour, and strongly reddens turnsole. On adding a little water the mixture thickens, and the ether swims on the top; but if a sufficient quantity of water be poured in to dissolve the ether, a body of oil is formed on the surface, bearing a considerable analogy to that of lintseed: this may be converted, by the assistance of the semi-vitreous oxide of lead, into an emplastic mass. Sulphuric ether the true solvent of birdlime. Water separates oil from the solution.

By evaporating the solution of birdlime in ether, a greasy substance is obtained, of a yellow colour, and of the softness of wax. Ether by evaporation gives a greasy substance.

Conclusion.

From the foregoing observations it will be perceived how little analogy exists between birdlime and gluten.

A simple comparison will be sufficient to designate the place it ought to occupy among vegetable productions. General properties of birdlime.

Birdlime

Birdlime is viscid, elastic, dries a little in the air, by exposure to which it becomes brown; but is not rendered brittle and irrecoverable like gluten.

It melts in the fire, swells, and burns with a vivid blaze; but does not diffuse that animal odour which is to be observed in gluten.

Water will not dissolve birdlime; it merely imbibes the mucilage, the extractive matter, and the acetic acid.

Alcalies dissolve it; when concentrated, they convert it into soap.

Dilute acids soften and partially dissolve birdlime.

Concentrated sulphuric acid renders it black and charry.

Nitric acid turns its colour to yellow, converting the substance partly to malic and oxalic acids, and partly to resin and wax.

Oxigenated muriatic acid renders it white and solid, constituting *oxigenated birdlime.*

Alcohol exerts but little action upon birdlime; it dissolves the resin and destroys the acid.

Lastly, sulphuric ether dissolves it entirely.

Recapitulation of the points wherein it differs from gluten.

Birdlime, therefore, differs from gluten,

1st. In the acetous acid which exists in it.

2d. In being very slightly animalized.

3d. In the mucilage and extractive matter which may be obtained from it.

4th. In the great quantity of resin which may be obtained from it by means of nitric acid.

5th. In its solubility in ether.

X.

Method of purifying Oil. By M. CURAUDEAU.*

The purification of oil consists in its clarification.

THE purification of oils for combustion consists solely in their clarification: It is only since Argand's lamps have come into common use that this subject has received much attention.

There are many processes for the purification of oils, but all are not equally good; and those who sell purified oil make a secret of the method of purification.

* From Cours complet d'Agriculture, Tome XII.

However,

However, as the art of purifying oils ought to be known by those who manufacture them, the processes, which are considered the most economical and simple, shall be here mentioned; by which information they will be able to obtain that profit which those now make who follow this species of industry after them.

The process for the purification of oil by sulphuric acid, which follows, is little different from that published by Thenard.

Process by sulphuric acid described.

To one hundred parts of rape oil one part of sulphuric acid is to be added, diluted with six times its weight of water; the mixture should be strongly agitated, and as soon as this is completely finished, it is left still till the oil becomes clear; when it is perfectly clear the purification is effected.

Sulphuric acid and water added to the oil; the mixture strongly agitated, and left to settle.

There remains at the bottom of the vessel an acid liquor somewhat coloured: the oil is to be separated from the sediment; and in order to be certain that no acid is retained by the oil, some ounces of powdered chalk is to be added; the mixture should then be shaken, and the oil again left quiet to settle.

The oil separated from the sediment; powdered chalk added; agitated again, and left to settle.

The action of the sulphuric acid in this process consists in depriving the oil of all its humidity, although it is itself mixed with water, and in separating from it a mucoso-extractive substance, the presence of which diminishes the energy of the combustion of the oil, covers the wick with charcoal, and produces much smoke: It is then on the abstraction of these principles foreign to the oil, that its quality of giving a good light depends.

The acid in this process separates the mucoso-extractive matter from the oil, which injures its combustion.

Another Method.

The next process to be described has been followed by some manufacturers, who have had good success with it.

Process by flour and water.

To one hundred parts of rape-seed oil ten parts of water are to be put, to which one part of wheaten flour has been added; the mixture is to be well agitated, and then to be heated until all the water added has been evaporated, or, more properly, until the oil has ceased to have any union with the substances which it held in suspension: In this state it becomes purified; and at the end of twenty-four hours it is very clear, and does not differ at all in quality from that prepared by sulphuric acid.

Flour and water added to the oil; the whole well agitated, and heated till a separation of the contained matters ensues; in 24 hours after this it is clear.

In

The heat should be applied gradually, and should not exceed 80° Reaumur.

In the practice of this last process, care should be taken to heat the oil gradually, and not to raise its temperature above 80 degrees of Reaumurs thermometer. (212 Fahrenheit) This heat is sufficient to effect the coction of the flour, and of the mucoso-extractive matter contained by the oil; a greater degree of heat would colour the oil, and deprive it of the appearance most favourable to its sale.

M. Curaudeau led to this process by observing the separation of white sauce into two substances when too much done.

M. Curaudeau was led to this process by an observation, which every one may likewise make. It is well known that the sauce called melted butter, when too much boiled is separated into two parts, one which is thick and occupies the bottom of the vessel, while the other part is clear and floats above the first: The lower substance is the caseous part of the butter united to the flour that has been added to the sauce, and which the action of the fire has separated from the oil,; The upper substance is the butter deprived of all foreign matter; and in this state it may be called *purified butter*.

XI.

On a peculiar Fluctuation of the River Dordogne, called the Mascaret. By M. LAGRAVE SORBIE *.

The mascaret takes place only when the waters are low.

THE peculiar movement of the waters of the River Dordogne, which is called the *Mascaret*, takes place twice each day in the summer time, *when the waters are low*, which is an essential condition. A similar motion also takes place on the river Amazons according to the report of M. de la Condamine, when it is named the Pororoca; the same is also perceived at the Orcades, off the north of Scotland, according this author: and M. Sorbie has seen accounts in the publications of some voyagers of its likewise occuring in some of the rivers of Hudson's bay, and also in the Mississipi.

The Pororoca of the Amazons similar to it, a like occurrence at the Orcades, and in the rivers of Hudsons bay, and in the Mississipi.

It is not surprising that this phenomenon does not happen in all rivers; it is not always seen even in the Dordogne. From the most exact observation, if the summer is not dry, and that the waters are not low to a certain degree the Mascaret does not appear, It rarely occurs in winter; it however

* Journal de Physique, LXI. 286,

sometimes

fometimes takes place during very hard frofts, when the cold has diminifhed the waters by the formation of much ice; but this happens very feldom, and has never been more than three times, in feveral ages.

The mariners foretel the Mafcaret from the lownefs of the river,

There is a *maximum* of depreffion in the waters neceffary to its appearance; Wherefore the mariners in the neighbourhood of Bourdeaux are in the habit of talking of it fomewhat in this manner, "The waters have fallen fo much, the tide will encreafe to day to fuch a height, we fhall have a Mafcaret", and they load their barks accordingly, and take precautions to avoid it. The manœuvres of thefe mariners have caufed fome naturalifts in the vicinity of Bourdeaux to obferve long fince, that this phenomenon muft depend on a natural caufe, connected with the bed of the river, fince thefe men can foretel, without being fcarcely ever miftaken, by the depreffion of the water, whether the Mafcaret fhall appear or not, although fometimes it has not appeared before for fome years, becaufe the rains have prevented the waters from diminifhing to the neceffary degree.

no account of the caufe of the Mafcaret yet publifhed.

No one has yet tried to explain the caufe of this fingular fact, not even M. Condamine, or if there be any works on the fubject, they are unknown to the author though his ftudies have been particularly directed in the line where fuch information might occur, and he has read much. In order to enable others to account for the facts, he mentions thofe which relate to it fuch as he has himfelf feen, and fuch as he has been told have been witneffed for feveral ages.

It confifts of an accumulation of water, which appears firft at Bec d'Ambes, and rufhes up the river with great velocity, and a frightful noife, terrifies the cattle, and the water fowl,

In the fummer, or, more properly fpeaking, when the waters are low, there appears at a little diftance from the junction of the Dordogne with the Garonne, or at Bec d'Ambes, an accumulation of water, like a promontory, on the fhore, which is from the thicknefs of a ton to that of a fmall houfe, and which rolls along with fuch velocity that no horfe, whatever might be his fpeed, could keep up with it. It follows the direction of the fhore, and makes a moft frightful noife. The horfes and oxen, which feed in the meadows near the river, run away with their utmoft fpeed exhibiting the greateft terror; fo much fo that they remain trembling a long time after, and cannot be driven back but with much difficulty. The ducks and geefe have alfo been feen to precipitate themfelves into the reeds at its approach, with the greateft fpeed and af-

fright

fright, and lie flat there, without being able to come out. Hard bodies, which lie in the way of the Mascaret are struck by it with such force, that the piers, built for the use of the vessels, along the shore are demolished, and some of the stones which compose them, although very large, are driven away more than fifty paces; the strongest trees are torn up by the roots, the barks which it meets are not only sunk, but broken asunder, especially if they are near the shore, or have any hard body lying beneath them. From the place called St. Andre (See the lower part of Plate IV.) on the river, the Mascaret forms itself into waves which half its breadth as far up as Caverne; there it disappears for a short time, to appear again between Asque and Lile like a promontory, and then returns into the form of waves as far on as Tersac; at Tersac it regains its first appearance, which it only quits at Vayne; from Vayne it proceeds along the bank as far as Fronsac, the house of M. de Richelieu; from Fronsac it occupies the whole breadth of the river, passes with a terrifying noise before the village of Libourne, throws the road for vessels belonging to this village into confusion, and afterwards appears at Genisac-les Reaux and at Peyresite with but very little force. The whole passes in the course of seven or eight leagues.

overturns the piers along the river, and drives the large stones which compose them, more than fifty paces off, tears up large trees by the roots, and sinks and breaks vessels in pieces. Above St. Andre it appears in waves, above Asque is seen in its original form; in waves again above Lile, at Tersac it regains its first appearance; at Fronsac it occupies the whole breadth of the river, passes before Lisbourne with a terrible noise, and ceases at Peyresite.

The following is the account, which M. la Condamine gives of the *Pororoca* of the river Amazons, the comparison of the effects of which with those of the Mascaret will tend to establish the theory of these phenomena.

Account of the Pororoca on the Amazons:

In his voyage to the river Amazons, page 193, he relates, that " between Macapa and Cape-Port, where the channel of the river is most confined by the islands, and especially opposite the mouth of the Arawary, which joins the Amazons on the north side, the flowing of the sea exhibits a singular phenomenon. During the three days next the full of the new moons, the times of the highest tides, the sea, instead of taking almost six hours to rise arrives at its greatest heighth in one or two minutes; it may be conceived that this does not happen quietly; there is heard at a league distance a terrible noise, which announces the *Pororoca*, which is the name that the Indians of these parts give to this frightful flood. In proportion as it approaches the noise encreases, and soon an accumulation of water, like a promontory, appears from 12 to 15 feet high; after that another is seen, then a third, and sometimes a fourth, which follow

its noise heard at a league distance. It advances in several waves, each twelve or fifteen feet high,

2 each

each other closely, and which occupy the whole breadth of the channel. These waves advance with a prodigious rapidity, break and overturn every thing which opposes them. I have seen in some places a large extent of land carried away, great trees torn up by the roots, and ravages of all kinds committed; every where that they pass the banks are swept clean; the canoes, the pirogues, and even the barks can only escape their fury, by anchoring in deep water. After having examined this phenomenon with attention in different places, I have always remarked that it only takes place, when the rising flood is engaged in a narrow channel, or meets in its way with a bank of sand, or a shallow place, which occasions an obstacle to it; that it was in those places alone that this impetuous and irregular movement of the waters commenced, and that it ceased a little beyond the bank, when the channel became deeper, or grew considerably wider. It is said that something similar to this happens at the isles of the Orcades, at the north of Scotland, and at the entrance of the Garonne, (it should be the Dordogne), in the vicinity of Bourdeaux, where the effects of these tides, is called a *Mascaret*."

which rush forward with great rapidity, and overturn every thing which opposes them, carry away large portions of the land, and tear up trees by the roots.

It only occurs in narrow channels, over sand banks, or shallow places.

It ceased where the channel became deeper or wider.

It appears from what has been cited from M. Condamine, that the effects of the Pororoca are almost the same as those of the Mascaret. Nevertheless there is a marked difference between them in this respect, that on the Dordogne, two kinds of floods take place, one which extends over the whole river, and is similar to that which M. Condamine has observed, and the other which ranges along the shore, rolling more over the deposits which the waters have left, than in the water itself. He says positively in page 194, that " at one or two leagues a frightful noise is heard, which announces the Pororoca; as it approaches the noise encreases; and soon an accumulation of water appears from 12 to 15 feet high, and then another that follows, which occupies the whole breadth of the channel". On the Dordogne the Mascaret rises with great noise, sometimes along the coast in an elongated accumulation, and sometimes in the form of frightful waves, which extend over the whole river; when it follows the shore it only appears in the re-entring angles, and on the sand banks, as is described in the sketch of the plan of the river, which accompanies this account, and which takes in the whole extent where the effects of the Mascaret are perceived. The parts covered with small

Difference between the Mascaret and Pororoca.

further particulars of the Mascaret.

Description of the sketch of the course of the

Mascaret in the river.

small points, indicate the sand banks where the Mascaret always commences; the parts occupied by small lines, are the places where the waves occupy the whole breadth of the river. The dotted parts indicate the re-entring angles, where the sand banks are found which are deposited by the counter current. It is here principally that the Mascaret rolls with all its fury over the mud of the river. On the banks the salient angles are the places where the Mascaret quits the shore, occupies the whole river, and runs upwards, accompanied by many considerable waves, which succeed each other, till another re-entring angle occurs, where it again resumes its first form.

It is thus that these who dwell in the vicinity of Bourdeaux witness without emotion twice each day, when the waters are low, so extraordinary a phenomenon, without any one thinking of examining into the cause of it, or even of communicating the particulars to naturalists.

The tide is the primary cause of the Mascaret.

The primary cause of this rising of the water is the same as that of the tide in all rivers; and if the Mascaret occurs on very few rivers, it is because their beds are not formed in a manner necessary to produce it, and have not the same disposition as the Gironde and Dordogne: they have either too little or too great a current; their waters are not sufficiently low, or when they are, the tide does not continue long enough; finally the re-entring and salient angles are not such as they ought to be. M. Sorbie thinks he could tell before hand whether any river would be liable to such effects, from the form of its plan and the disposition of its bottom; and is of opinion that the cause why more rivers are not subject to the Mascaret, depends entirely on the shape of their beds, and not on any particularity in their tides. The physical cause of that on the Dordogne appears very simple, M. de la Condamine says, that on the Amazons it is always at the narrow parts where it is observed. The cause is not the same on the Dordogne, for there is no narrow parts in almost its whole course: it is nearly every where very rapid, and of small depth, as all those rivers are which have much current. It forms, as may be seen in the plan, many turns and windings; and has few isles: but at each angle a bank of sand is deposited: It descends, notwithstanding these windings, almost from the east to the north-west. As far as Bec d'Ambes, where it unites with the Garonne which is much more powerful than it, and they form together that beautiful arm of the sea, called the

The course of the Dordogne described, to account for the Mascaret,

the Gironde. The two rivers then defcend together from Bec d'Ambes to the fea in a direction from the eaft to the north-weft. All the waters which arrive from the arm of the fea or from the river, advance in a ftraight line with abundance into the mouth of the Dordogne, inftead of mounting up the Garonne, which runs almoft north and fouth as far up as Bourdeaux. The greateft part of the waters which are advancing to the Garonne, ought then, when the current has taken its courfe, to run up the Dordogne at the beginning of the flood, fince its velocity does not allow them time to turn up the Garonne; and thus the water which ought to go to the Garonne, running up the Dordogne, form by their abundance, this effect which Condamine recites: He fays that "the tides, which ufually take fix hours to rife, arrive at their full height in one or two minutes". But on the Dordogne, the tides never come to their higheft level in near fo fhort a time, even when the waters are loweft; but in one or two minutes they encreafe confiderably; which encreafe is probably caufed by the waves which arrive almoft inftantly; and the flood raifing their maffes of water above their natural level, leaves them there to augment the water in the bed of the river in proportion to their bulk. After the Mafcaret has paffed, the waters of both rivers encreafe in the fame gradual manner as thofe of all other rivers.

fuppofed to be caufed by the waters on their way to the Garonne, taking the ftraighter courfe up the Dordogne.

M. Sorbie likewife thinks, after all, that the tide of the Gironde may be the caufe of the Mafcaret on the Dordogne, for it pours its waters into the mouth of the Dordogne in almoft a right line; this arm of the fea being at leaft fix times larger and deeper than the Dordogne, ought at the flood to carry up fuch an abundance of water, as could not enter into the bed of this river without occafioning the accumulation of waters defcribed. The phyfical caufe then of the Mafcaret is the confiderable mafs of water which arrives from the Gironde into the mouth of the Dordogne, and the fmall depth of this river; fince it is known that in rainy feafons, and when the river is a little encreafed in fize, this circumftance never takes place.

It may alfo be caufed by the tide of the Gironde rufhing in a right line into the mouth of the Dordogne, and by the fhallownefs of this river,

M. Sorbie remarks in conclufion that the facts related fhew evidently that the flowing and ebbing of the tides of rivers are different from thofe of the fea; that the ebbing and flowing of rivers, are only fecondary effects of the tides of the fea; that is to fay, that the waters of the fea only form a dam to thofe

remarks on the tides of rivers, fuppofed to be caufed by the tide of the fea forming a dam acrofs their courfe.

thofe of the rivers, and that the rivers form by the abundance of their waters, thofe rapid flood-tides which are obſerved on the great rivers, fuch as thofe of the river Amazons, which afcend from 5 to 100 leagues, thofe of the Senegal, which advance almoft as far up, and thofe of other rivers almoft equally confiderable. M. Sorbie thinks that the Mafcaret, or the Pororoca, have altogether the fame caufe as the flood-tide of rivers, and though fome flight fecondary effects occur, fuch as thofe related, that all arife from the fame phyfical caufe.

XII.

Defcription of a fecret Lock of ten thoufand Combinations. W. N.

Difquifition upon locks. The common lock.

THE common lock ufually confifts of a bolt, which requires a particular inftrument, called the key, to pufh it backward and forward; and in order that this bolt may be inacceffible to violation, certain impediments or obftacles, ufually called wards, are interpofed between the key-hole and the bolt, which make it difficult to open the lock by any general or common procefs. The general procefs for picking a lock, of which the key has not been feen, confifts in operating upon the bolt by a fmall bended inftrument or wire; or elfe by endeavouring to difcover the pofition of the wards by an unperforated key, on the face of which fome foft or plaftic matter is lodged. And when this fituation is once difcovered, it is not difficult to file away fo much of the key as fhall allow it to pafs, or elfe to felect, out of a number of fkeleton keys, one, of which the form fhall admit of its paffing through the lock. There are many locks fo fituated, as for example in the veftries of churches and other little frequented places, as to admit of this flow operation; but it muft at the fame time be allowed, that the Englifh market prefents locks of a number of different conftructions, which can neither be picked nor analyzed by the procefs here mentioned. Nothing is more common, however, than for keys to be entrufted out of the hands of the poffeffor, or to be hung up, or cafually laid down or millaid. In thefe circumftances their figure may be taken with

Bolt, key, wards, pick-locks, fkeleton-keys.

with wax, like the impression of a ſeal, or more ſpeedily by indentation upon a piece of moiſtened paper, or by various other means; and it muſt be admitted, that very little ſkill is required to enlarge the openings of a common key, ſo as to make it paſs the wards of a ſuperior lock.

Theſe neceſſary and unavoidable imperfections of common locks, have long ago led to the introduction of ſecret locks, which are ſo conſtructed as to require ſome particular manipulation in opening them; ſuch as that the key ſhould be turned twice round, or that it ſhould be turned through a certain ſpace in one direction, and then back again; or that it ſhould act upon ſome delicately reſiſting piece, very likely to be diſregarded by an uninſtructed poſſeſſor of the key; or that a number of viſible parts ſhould be placed in ſome determined order, before the common proceſs of opening, either with or without a key, can take place. Upon all theſe contrivances one general remark may be made, namely, that the poſſeſſor muſt always in perſon open his own lock; for if this be to be done by the mere practice of a ſecret without a key, his cabinet becomes for ever open to him who, by communication or otherwiſe, ſhall poſſeſs that ſecret; and if a key be uſed, his lock, as to that perſon, becomes as ſubject to violation as a common lock.

Secret locks; their ſtructure and imperfection.

In the mechanical conſideration of a ſecret lock, we may ſuppoſe the conſtruction to be entirely unknown to him who is deſirous of opening it. In this, according to the experience and ſagacity of the operator, the difficulties will be greater or leſs, and a very ſhallow contrivance may occaſionally preſent a greater obſtacle than a much more elaborate ſtructure. But if we ſuppoſe the ſyſtem of the lock to be known, but the particular conditions of opening it to be ſecret, the examiner will then take for his guide the probable circumſtance that the re-action of the parts may feel conſiderably different, when they are duly placed for opening, than when their ſituation is ſuch as to prevent that effect. By this clue, and by careful examination, moſt of theſe locks may be opened; and it is remarkable, that the better the workmanſhip the more eaſy it is in general to make the intended diſcovery.

Methods of violating them.

The following are the conditions which appear to me to be neceſſary in a lock of the moſt perfect kind:

Conditions of a perfect lock enumerated.

1. That

1. That certain parts of the lock ſhould be variable in poſition through a great number of combinations, one only of which ſhall allow the lock to be opened or ſhut.

2. That this laſt mentioned combination ſhould be variable at the pleaſure of the poſſeſſor.

3. That it ſhall not be poſſible, after the lock is cloſed and the combination diſturbed, for any one, not even the maker of the lock, to diſcover by any examination what may be the proper ſituations of the parts required to open the lock.

4. That trials of this nature ſhall not be capable of injuring the work.

5. That it ſhall require no key;

6. And be as eaſily opened in the dark as in the light.

Theſe conditions are in ſome reſpects liable to the inconveniences already mentioned. I would therefore add the following conditions:

7. That the opening and ſhutting ſhould be done by a proceſs as ſimple as that of a common lock.

8. That it ſhould open without a key, or with one, at pleaſure.

9. That the key-hole be concealed, defended, or inacceſſible.

10. That the key may be uſed by a ſtranger without his knowing or being able to diſcover the adopted combination.

11. That the key be capable of adjuſtment to all the variations of the lock, and yet be ſimple.

12. That the lock ſhould not be liable to be taken off and examined, whether the receptacle be open or ſhut, except by one who knows the adopted combination.

Deſcription of a new lock of combination.

In meditating upon this mechanical problem, I have thought of various conſtructions, but have not yet matured one in which all the above conditions are complied with. The lock delineated in *Plate III.* poſſeſſes the firſt ſix requiſites. *Fig.* 1. repreſents the plate of the lock, of which the other ſide is ſeen at *Fig.* 4. In this laſt figure the middle piece is a handle or knob, repreſented *Fig.* 6, which, when turned, ſerves to ſhoot the double bolt *k*, *Fig.* 1, by any common connection. In the actual lock this bolt is carried backward and forward by a pin ſtanding out of *Fig.* 2, ſoon to be deſcribed. The other four circles in *Fig.* 4, are handles, repreſented in *Fig.* 5, which ſerve

ferve to move the four wheels feen in *Fig.* 1. Thefe wheels have twelve teeth each, and are faftened by center-fcrews, each upon a flat wheel of the fame tooth; but having only ten notches actually cut, as is feen in the right hand upper corner, where one of the upper wheels is taken off, and is fhewn at *Fig.* 3. Thefe upper wheels have their toothed part confiderably higher than the interior or flat part; fo that they would be contrate wheels if the teeth were cut quite through. But this is not the cafe, except with two of the notches, as may be feen in the two lower wheels more particularly, and alfo in the others. The upper wheels have alfo two of the notches between the teeth ftopped up, as is fhewn in *Fig.* 3; by which contrivance there are but ten fituations for fcrewing each wheel upon its correfpondent under wheel; and thefe fituations are rendered precife, and all relative motion between the two correfpondent wheels prevented by a fmall ftud feen in the uncovered wheel, *Fig.* 1, which fits into one of the notches of the upper wheel when put in its place. The upper wheel has a number on each tooth from 1 to 9 and 0, which are of ufe for placing this ftud. The four under wheels are held in their fituations by four fpring-catches, which allow them to be turned, in one direction only, by means of their knobs or handles; and when any wheel is thus turned round, the finger and thumb will feel the ftroke of the lever, as it fucceffively falls into each notch, until the lever comes to reft upon the fmooth part. This very palpable indication then fhews when to begin to count, calling the firft hold or ftroke of the catch 1; the fecond 2; the third 3, &c.; and the lock is fo conftructed, that when the top wheel of any of the four couple is put on with any number oppofite the ftud, the fame number counted by the catch will place the upper wheel in fuch a fituation, as that its notches, which pafs clear through, will lie in a circle defcribed from the center or axis upon which the great handle turns. And therefore, when each of thefe wheels is put in its place, and the numbers known (and regiftered, or put in the memory by fome artificial affociation, fuch as of the date of the year taken either backwards or forwards, &c.) it is only needful to move each of the four knobs till its catch has paffed the fmooth part, with a number of ftrokes anfwering to its adjuftment, and the circle indicated by broken fhaded lines in *Fig.* 1, will be capable of paffing through

Defcription of a new lock of combination.

Description of a new lock of combination.

through the open ſpaces of every one of the wheels. *Fig.* 2. repreſents a contrate wheel, having its irregular portions A, B, C, D, &c. ſtanding up above its plane. Theſe portions are parts of a circle equal to that denoted by the broken ſhaded parts in *Fig.* 1. The contrate wheel is to be placed in *Fig.* 1. with its face turned down; and being there ſcrewed with its center to the central handle, it ſerves to open and ſhut the bolt, which it can only do when the four wheels are in ſuch a ſituation as to allow the circular edge-parts of *Fig.* 2. to paſs clear through their notches. If any one or more of thoſe wheels be turned ſo as not to correſpond with its number, it will be impoſſible to turn the handle, becauſe every attempt to do ſo will cauſe one of the parts of *Fig.* 2. to ſtop in one of the notches of the wheels through which it cannot paſs. The method of opening the lock will therefore conſiſt in ſetting each wheel to its known number.

As the proper ſituation of each wheel is only one out of ten, it is nine to one againſt any operator upon this lock, that he ſhall not ſet the firſt wheel right, ſuppoſing all the others in their due poſitions; but it is true that he may try all round, and will come to the right place at laſt. If two only of the wheels were deranged, it would be eighty-one to one that he ſhould not ſet them both right; and he would be deprived of any trial round a ſingle wheel, becauſe the other wheel would always hold againſt him, and prevent his knowing when the open notch of the wheel under trial preſented itſelf. Three wheels deranged would make the odds 729 to one, and the four would make the odds 6561. In the plate the combinations are ſaid to be ten thouſand, from an overſight in taking the ratio of ten to one inſtead of nine to one. But this is a matter of no conſequence as to the principles of the lock, becauſe the number of teeth or number of wheels are capable of variation. If a fifth wheel were added to this lock, the odds would amount to 59049.

As the quantity cut from *Fig.* 2. is not more than was neceſſary for the clear rotation of the wheels when the lock is ſhut, this piece, when in every other poſition, prevents the other wheels from being turned at all.

Letter

XIII.

Letter from Mr. ALEX. CROMBIE, *concerning the Caledonian Literary Society at Aberdeen.*

To Mr. NICHOLSON.

SIR,

THE want of focieties for fcientific and literary improvement, has been long felt in many confiderable towns in Scotland, and I believe in none more than in Aberdeen.

The utility of fuch inftitutions being fo generally acknowledged, it is truly a matter of furprize to find fo few of them in this kingdom, efpecially when the facility of forming them is confidered. Any attempt, however fmall, to promote the interefts of literature, and to diffufe moral, political, or philofophical knowledge among men of all ranks, will ever meet with the marked approbation of the fincere wellwifher to his country; and I am perfuaded you will receive peculiar fatisfaction in being able to communicate to the public the feebleft efforts which may be at any time directed to fo important and defirable an object. Great utility of focieties for fcientific and literary improvement.

In your Journal for December laft, a *traveller* has expreffed his furprize to find no antiquarian or literary fociety, or fubfcription library, at Aberdeen; and I agree with his remark, that thofe who know the refpectability of the place, cannot fail to be aftonifhed at it. To account for fo fingular a fact would perhaps be deemed prefumptuous. I have too much refpect for my fellow-citizens to attribute it to a want of tafte, but I cannot help blaming thofe amongft us who are qualified for fupporting fuch inftitutions, for their want of attention in this refpect. Reference to a letter in a former Journal.

The Profeffors of both Univerfities certainly unite talents with influence and refpectability,—It were to be wifhed that they and other literary characters in town, had more concern for the improvement of the community at large, and would make fuitable efforts to promote it.

It would be doing injuftice to the liberality of the proprietors of the Athenæum and circulating library, to deny thefe inftitutions their refpective merits and advantages. But I apprehend that neither of them is fufficient to fupply the *de-* The Athenæum and circulating library.

fideratum

ſideratum mentioned by your correſpondent. The firſt is principally calculated for the commercial part of the inhabitants, and thoſe who have time to *lounge*; the ſecond, although compriſing much uſeful reading, is ſometimes defective in the ſelection of the books, and affords little opportunity for the union of literary exertions.

Conſideration in favour of a proprietory aſſociation.

A ſociety whoſe books are the property of the individual ſubſcribers, is far better adapted, not only for advancing knowledge and bringing uſeful talents into notice, but alſo for giving a favourable bias to the purſuits of ingenious young men of all deſcriptions, to whom ſuch a ſociety is at all times acceſſible, from the ſmall expence attending it. People become more ſolidly concerned in promoting the ſucceſs of any ſcheme, in proportion as their perſonal intereſts are interwoven with it; and we may therefore conclude, that a man will take more pleaſure, and perhaps derive more profit, from reading a book which he conſiders as his own property, than one only lent him for a time.

Subſcription-library eſtabliſhed Feb. 1805,

Impreſſed with theſe conſiderations, a few perſons in Aberdeen inſtituted a ſubſcription-library upon the 22d February, 1805, under the title of the *Caledonian Literary Society*. Beſides embracing all the periodical publications of merit in Great Britain, our ſtock is enriched with a ſelection of the moſt approved books, either preſented by the members or purchaſed from the Society's funds: Which Society has *already* increaſed to upwards of 100 members, and the liſt is daily augmenting in number and reſpectability.

at a very moderate expence.

It is worthy of remark, that the trifling ſum of ſix ſhillings *per annum* is only required from each ſubſcriber to *The Caledonian Literary Society*. So inconſiderable an expence, contraſted with the great variety of uſeful and entertaining knowledge to be derived from it, muſt form a very powerful recommendation in its favour.

We have been informed with pleaſure, that many perſons in Glaſgow, who are not members of the Society eſtabliſhed there, have contributed liberally to its ſupport by giving books —an example worthy of the imitation of others.

A Philoſophical Society in contemplation.

It is alſo in contemplation to inſtitute a Philoſophical Society, on a plan ſimilar to thoſe of London, Edinburgh, &c. for the purpoſe of receiving occaſional diſſertations on a variety of literary

literary and other fubjects, to be depofited as the property, or, entered into the books of the fociety; and afterwards publifhed in fuch manner as the fociety may direct.

Should any of the friends of fcience in Invernefs, Banff, Peterhead, or other places, be defirous of eftablifhing fimilar inftitutions, we will moft chearfully furnifh them with a copy of our plan and regulations.

We have a fincere wifh to fee every encouragement given to undertakings fo laudable and beneficial, and have with this view made the prefent communication, to give publicity to ours through the medium of your excellent Journal. The infertion of the above will oblige, Sir,

With refpect,

Your humble fervant,

ALEX. CROMBIE, *Pres.*

Aberdeen, January 2, 1806.

XIV.

Letter from Mr. James Stodart, *in Anfwer to a Queftion concerning the Effect of the Nitrous Oxide, propofed by Dr. Beddoes.*

To Mr. NICHOLSON.

Dear Sir,

Dr. Beddoes, in a paper on the medical effect of refpiring the nitrous oxide, publifhed in the laft number of your Journal, refers to an account I formerly gave of fome unpleafant and rather alarming fenfations experienced after inhaling that gas. He attributes the whole to hyfteria or nervous affection; at the fame time fignifying a wifh that I would ftate whether or not that was really the cafe. In anfwer to this I have only to obferve, that if any fuch predifpofition to hyfteria did exift, it was wholly unknown to me. My general ftate of health was as ufual; nor had any thing occurred particularly to affect the mind. I had often inhaled the nitrous oxide under circumftances in every refpect fimilar (at leaft as far as I can judge) and till that time, fo far from experiencing any thing like

Qu. Whether Mr. Stodart was nervoufly affected previous to his feeling inconvenience from nitrous oxide.

Reply: that he was not.

like debility, the very contrary effect was produced; namely, sound and undisturbed sleep in the night, followed by strength and increased chearfulness on the following morning.

Expectation that the nitrous oxide may prove eminently useful, &c.

I very sincerely hope the medical application of this extraordinary agent, directed as it is by the very able hand of Dr. Beddoes, may prove as important and useful in medicine as it is interesting and curious in philosophy.

I have not yet heard of its being tried in cases of suspended animation; it appears to be an experiment well worth making. The subject is perhaps worthy of the attention of the Humane Society. I am with respect,

Dear Sir,

Your's sincerely,

JAMES STODART.

Strand, January 22, 1806

XV.

Description of a Statical Lamp, which maintains a Supply of Oil to the Burner from a Reservoir, placed so low as to occasion no Interception of Light. By A. F.

To Mr. NICHOLSON.

SIR,

Description of a new statical lamp.

I SEND you a sketch of an overflowing lamp, of which the construction will be easily deduced from the figure. Its advantages are, that the flame is supplied from below, and the light is not intercepted, but falls on all surrounding objects as directly as that of a candle. The upper part of A (see Plate IV.) contains the usual apparatus of a lamp, either according to Argand's construction or any other; and the column or tube which supplies the oil may be no longer than that supply and the conditions of the structure may demand. The vase below contains the oil, which is poured in, when needful, at the top of the column, by a funnel or otherwise. The circle round B, C, represents a globular (or cylindrical) vessel, having no communication with the vase except through a neck

a neck or pipe D, proceeding downwards nearly to its bottom; but there is a communication with the external air, through a perforation (reprefented by a fmall fhaded circle near B) which prevents the atmofphere from interrupting the intended action. The lightly fhaded femicircle B reprefents an hemifpherical folid capable of revolving on an horizontal axis, fo as to hang downwards and fill the lower half of the globe, when no fluid is prefent; or it can be raifed up by floatage into any other pofition, according to the quantity and denfity of any fluid that may be poured in. Description of a new ftatical lamp.

Let us now fuppofe the veffel C to contain any fluid not more than half its capacity, and that the revolving piece B is of fuch a weight as to be of half the fpecific gravity of that fluid: it may then be eafily underftood that the piece B will fettle into fuch a fituation as that part of it fhall be immerfed in the fluid and fupport it in the veffel, exactly to the height of its axis. For the part of the folid, immerfed on one fide, is exactly equal to the fpace above the fluid in that fituation, on the other fide; and the greater part of B which is on one fide of the perpendicular will exceed the fmaller part on the other fide, by exactly double that quantity. Confequently the immerfed part of the folid will be preffed down by twice its own weight; and this is exactly equal to the weight of fluid which it difplaces; whence the body and the fluid will be in equilibrio. Let us now fuppofe the fluid to be brine, at the fpecific gravity of 12, which may be poured in either at the top or at the fide hole, and that oil of the fpecific gravity of 9 be then poured upon it; and it is manifeft that the oil will prefs the denfe fluid upwards into C, as reprefented in the figure, and that when C is half filled, the oil will ftand at an elevation above the axis equal to one half more than the height of the denfe fluid, meafured from its furface where the oil preffes upon it. And, when this adjuftment is once made, by putting in the proper quantity of denfe fluid, if any of the oil be taken out, or confumed by burning, the preffure will be lefs, and the denfe fluid will rife within the vafe. But this rife will not be attended with any depreffion in the veffel C, becaufe the level will be kept up by the revolving piece B, and confequently the oil itfelf will be prevented from falling as much as it would have done if this contrivance had not been applied.

I do

Defcription of a new ftatical lamp.

I do not difguife the confideration, that as the oil diminifhes, the diftances between the upper and lower furfaces of the denfe fluid muft diminifh, and a proportional difference or fubfidence in the furface of the oil muft take place. The proper remedy for this appears to be that the lower furface fhould be made as large as convenience will allow; that its rife and fall may be lefs.

With regard to the difpofition and form of the fpaces which are to contain the oil, it is only needful to obferve that they may all be made fmall or narrow, except that which is alternately to be occupied by the oil, and the denfe fluid. If the height of the denfe fluid be 12 inches, the lamp may ftand 18 or 20 inches high, ufing falt water as above mentioned.

There are various practical objections to mercury; but if this fluid were to be ufed, the oil might be raifed ten times as high, or the apparatus, if required, might be conftructed with a lefs diftance between the furfaces.*

I am, Sir,

Your conftant Reader,

A. F.

XVI.

Letter from a Correfpondent rectifying fome Particulars of Mifinformation refpecting the Fifhery of the North of Scotland.

To Mr. NICHOLSON.

SIR,

WHEN any important information is communicated to the public, we have a right to expect that it fhould be given with extreme accuracy; or at leaft where any doubts exift, with fuch a degree of diffidence and modefty, as may leave room for avoiding mifreprefentation or falfhood.

* The contrivance for keeping a fluid at its level by a femi-circular revolving folid was invented by Robert Hooke. See Birch's Hiftory of the Royal Society. A. F. has ingenioufly adopted it to a lamp which cafts no fhadow. Hooke's lamp is nearly as faulty as the common fountain lamp in this refpect. N.

I wifh

I wish *an Enquirer*, in your Journal for December last, had attended to this, before making what I conceive to be a hasty, ill-founded statement, respecting *certain instances of wasteful negligence in some of our fisheries in the north of Scotland*, which it is my duty at present to controvert.—He states: Erroneous information respecting the fisheries on the north of Scotland.

1st, "That the fishermen of Aberdeen, Banff, Peterhead, &c. never think of carrying their fish along the coast southward, which they might do to Leith in twenty-four hours; or with a good brisk wind to Berwick-upon-Tweed, or even Newcastle-upon-Tyne; but when their respective towns are supplied; *they throw the remainder upon the dunghill for manure!!*"

A fact so improbable as the above, would indeed, require no ordinary share of proof to gain credit to it, and I have the satisfaction to assure you that it is entirely without foundation. The truth is, the number of hands employed in the fisheries in the north of Scotland are so few, and the encouragement given to enterprize and speculation in this important source of national wealth so small, that no more fish is caught than what supplies the neighbouring towns. But even admitting that more were caught, and that we could vend at Leith, Berwick-upon-Tweed, or Newcastle-upon-Tyne, is it not to be supposed that fishers of places nearest to these towns could afford to greatly undersell us? From various causes this source of wealth is neglected.

When the dog-fish (*squalus catulus*, L.) appear on the coast, our fishers catch a great number of them and dry them for their own private use (for none but themselves and the lower classes of people would use them) and likewise for the benefit of the oil, which they yield in great abundance, and the skin, which is used for smoothing the surface of wood. After they are drained of the oil which they contain, besides keeping a sufficient number for use, they throw the remainder on their dunghills, which produces a *valuable* manure. And no doubt your correspondent may have mistaken these for any other kind of fish. The dog-fish caught for food, for oil, for its skin, and for manure.

He next observes; "That at Arbroath, another custom, equally as extravagant in its kind prevails, and of which I have been a witness; the crab fishery is so productive, that after boiling them, the bodies of the crabs are thrown away, and the *large claws only* brought to table."

It is

The claws of crabs only are sold at Arbroath, but the bodies are not thrown away.

It is indeed, generally the case here, and in every other fishing town, that the fishers for the most part retain the bodies of the crabs, and only dispose of the claws in the public markets: but that the former are thrown away, is by no means true in almost any instance; for the fishermen find them of far more value in baiting their hooks, than what they could get for them otherwise. Indeed, if it were not for this purpose, it is believed, few or no crabs would be caught at all.

Much profit might be derived by a company if established at Aberdeen for exporting white fish.

Having thus endeavoured to vindicate our fishers from the charge of wasteful negligence, which none who know them will think them guilty of; I cannot conclude without expressing my surprize that no company has yet been established at Aberdeen for exporting white-fish. It is obvious from its excellent situation, and advantages, that very handsome profits could be cleared, if such an undertaking were once set on foot, and well conducted; equal, if not superior to the salmon fishing, which it is well known has been greatly the means of enriching this place.

If you deem the above observations worthy a place in your useful Journal you will oblige,

SIR,

Yours respectfully,

A. L.

Aberdeen, January 3, 1806.

XVII.

Observations and Enquiries concerning the Heat of Air blown from Bellows. By K. H. D.

To Mr. NICHOLSON,

SIR,

Passage from Dr. Black's lectures.

I BEG leave to mention a passage in Dr. Black's Lectures on the Elements of Chemistry, published by Professor Robison, which occurs at page 88, Vol. I.

The author is speaking of the communication of heat, and has, in the former part of the page accounted for the apparent coldness of a stream of air, by its preventing the accu-

mulation of heat around our bodies, by its impulse and rapid succession, both cooling our clothes faster, and carrying away the warm air that was intangled in them. The Doctor says, "the sensation of coldness, therefore, produced by wind, or agitated air, is so much stronger than that produced by equally cold air in a stagnating state, that we are often persuaded the agitated air is actually colder, until we examine it by the thermometer; and Dr. Boerhaave thought the deception so strong, that he contrived an experiment to remove it completely (Boerhaave Elementa Chemiæ.) He suspended a thermometer in the air of a large room for some time, and noting the degree to which it pointed, he then directed against the bulb of it a stream of air impelled by a large bellows in the same room;—that stream of air would certainly feel to a person who opposed any part of his body to it, considerably colder than the rest of the air in the same room; *but the thermometer is not in the least affected by it.* And it would be easy to exhibit another experiment to shew, that agitated air is not made colder by agitation. A piece of ice, for example, being suspended in the air of a warm room, and blown upon by bellows, instead of being thereby kept the more cool, as our hand would be, and preserved the longer from being totally melted, would certainly be melted so much the faster, than when the air is allowed to stagnate in some measure around it."

that agitation of the air, though it cools heated bodies, does not render the air colder,

nor hotter, though it melts ice.

I take the liberty of troubling you with this in consequence of a communication from your ingenious correspondent, Mr. Richard Winter, published in the last Number of your excellent Journal, where his experiment on the effect produced on a thermometer by a blast of air from a pair of bellows, directly contradicts Dr. Black's assertion, that "the thermometer is not in the least affected by it."

M. Winter found the air from bellows gave out heat.

That there is great truth in Dr. Black's general statement of the fact, of a blast of air cooling a body warmer than itself, by affording a continued series of fresh surfaces to carry off the caloric, I have no doubt, and that it should have an equal effect in warming a body colder than itself, seems equally evident, or by supplying the colder body with caloric. But in the case of the thermometer being raised four degrees, (as stated in Mr. Winter's experiments) we are not told that it was of a temperature lower than that of the air of the room

Questions respecting these facts.

room. How then, Sir, are we to reconcile the refult of your correfpondent's experiment with Dr. Black's affertion, mentioned above?—Are we to fuppofe the blaft of air to have actually acquired an increafe of temperature, and if fo, how has it acquired it? I hope your correfpondent (fhould this ever reach his ears) will not imagine I doubt the accuracy of his experiment; my only object is, the clearing up a circumftance, which at prefent is to me at leaft, not by any means fatisfactorily accounted for. To whom then can I better apply, than to you, if indeed I may venture to hope you may think the object worthy of your confideration? Whether that fhall prove the cafe or not, I muft always feel (in common with thoufands of others) the benefit you confer on the fcientific world, by the eafy means of communication of knowledge to the public, which your Journal affords,

I have the honour to be,

Sir,

Your obedient Servant,

K. H. D.

Tunbridge,
January 19, 1806.

P. S. I do not underftand how the fuppofed greater capacity of a vacuum for caloric explains the facts, whether of the rife of the mercury in the thermometer, or the melting of the ice.

Obfervations on the preceding Letter, by W. N.

It is defirable that the experiments fhould be repeated.

WHEN a queftion arifes concerning the difagreement of facts, the procefs obvioufly indicated is to repeat the experiments; in order that it may be feen what circumftances may have tended to produce miftake, or what may have been the real difference between operations fuppofed to be the fame. Though I have not had an opportunity of doing this, I have neverthelefs thought it proper to make a few remarks.

Agitation enables a fluid to gain the common temperature more fpeedily.

When a body is immerfed in the air, or in any other fluid differing from itfelf in temperature, the body will acquire the common temperature more fpeedily (that is to fay, it will be heated or cooled more quickly) by agitating the fluid, than if it were left undifturbed;—and this for the plain reafon, that more of the particles at the original temperature will come into

int contact with it in the latter than in the former case. These remarks support and explain the facts noticed by Dr. Black and Boerhaave. Agitation of the air is merely supposed, and not that it shall be either condensed or rarified. Many facts concur to shew, that the capacities of elastic fluids for heat are encreased by rarefaction, and diminished by condensation; proofs of which we have by experiments in the air-pump and condenser, and in the late experiments of explosions produced in the chamber of the condensing syringe. If we attend to this law, we must infer that the air in a pair of bellows, being suddenly compressed by a force perhaps equal to one twentieth of an atmosphere or more, will acquire an increase of temperature; and if in this disposition to give out heat, it be made to rush against the ball of a thermometer, it will heat the mercury, and cause it to rise in the tube. Now, in order to reconcile both the results of Mr. Winter, and of Boerhaave to truth, we must recollect that bellows, like the unfortunate traveller in Esop's Fables, can blow hot and cold at the same time. If the thermometer be held very near the aperture, the warm air will heat the mercury; but if it be held at a greater distance, where the warm air has become plentifully mixed with cold, the effect of its temperature may be altogether inconsiderable, while that of the agitation continues to be effective: that is, to say, the thermometer if already at the common temperature, will neither rise nor fall; if it be already hot the steam will cool it; or if cool the steam will heat it. Thus it is, to return to our traveller, that we breathe upon our fingers held close to our mouth when we mean to warm them; but when we wish to produce cold, we hold the subject at a distance, and blow at it.

Air is heated by condensation.

Whence the rear blast of bellows will be hot, and the remoter will act only by its motion.

As the thermometer falls in the pneumatic vacuum, I suppose there may be some mistake in the postscript.

XVIII.

Account of the Performance of the patent Ship Economy at Sea, in a Voyage to the West India Islands, and of some Improvement in the Tackle aboard, proved of great Utility. By Mr. J. Whitley Boswell.

To Mr. NICHOLSON.

Dear Sir,

Description of the ship's construction has been published in a former number of this work.

As in a former number of your Journal *, you favoured me by inserting a description of the construction of the ship Economy, built according to my patent, I hope you will also admit the following account of her performance at sea, and of some other matters; of considerable utility to naval concerns.

The subject proper for the Journal as containing an account of an experiment in the arts important to the nation, and on a great scale.

Your Journal is principally devoted to the furtherance of the most useful of all knowledge, that of experiments in Philosophy and the Arts. And to a nation which like this depends on its shipping for most of the many advantages it enjoys over the rest of the world, what experiments can be more important, or ought to be more interesting, than those which concern this subject?

The experiment which has been made on this occasion is entitled to a farther superiority over other usual experiments, an account of the large sum of money required for conducting it, which altogether rather exceeded 5000*l.* and on this occasion it is but justice to mention the spirit with which Wm. Lushington, Esq. of this City, and Richard Griffith, Esq. of Dublin have came forward to assist in making this experiment, whose property the ship principally is, (my share of it being comparatively small to theirs); to those gentlemen this country is chiefly indebted for proving a matter of great utility to its naval concerns, and which sooner or later must be of the greatest advantage to it, when the plan comes into use, though the spirit of the times may defer this period until it shall cease to be of any benefit to us, and others may reap the profit of these gentlemens public spirit and my labour and study; but as I waited till I should have the proof of actual experiment to add, to that of a theory (which though founded on un-

Gentlemen who have assisted in this experiment.

The plan must be of great use to the nation when adopted,

* Vol. IX. p. 166.

erring

erring principles, and of which each part had been often proved in detail before, it could not be expected to convince those whose pressure of business, or want of taste for such studies, deprived of time, or inclination, or made it too great a labour to attend to its demonstration in any other way) I shall hope now, (that my exertions to bring this plan of ship-building into the notice it deserves, when its sufficiency, strength and security is,) supported by actual and severe proof, will meet with a fair and candid consideration, from the direction of our navy, and those whose commercial pursuits lead to employ vessels of great burden.

hopes that the success of the experiment will accelerate this period.

The chief advantage of this method of ship-building is, that it enables the builder to use timber of much less cost, and vastly more easy to procure, with strength and stability superior to the old method, in proportion to the quantity of timber, and to dispense with knee timber entirely.

Economical advantages of this method of ship-building, cheaper timber, more easily procured, may be used in it.

In a national point of view this method is still of greater benefit; for as it admits of timber of fifty years growth to supply the place of that of one hundred, not only the forest lands may be made to produce timber for double the number of ships for our navy in a given time, but private gentlemen would be also induced to plant more timber for this purpose, from the superior profit they could in this case make of their plantations, and the hope it would give them of being able to receive the fruits of their labour during their own lives, which at present can only be expected to be reaped by their grand children.

Its national advantages, the forests could supply double the quantity of the timber wanted in this plan in a given time, more timber would be planted if it was adopted; oaks of fifty years, have more serviceable timber in proportion than those of 100, and occupy only one fourth the ground.

An oak of fifty years growth has also a much greater quantity of serviceable timber in it, in proportion to its age, than one of an hundred years, and four times the number of them at least can stand and flourish at one time at the same extent of ground; so that the public would be benefited by the adoption of the plan every way; for while timber would thus be rendered more plenty, those who prepared it for market would also obtain a greater profit.

Hitherto the price of timber for the navy has been attempted to be kept down by arbitrary regulations, which tended to encrease its scarcity; at last, notwithstanding every effort, the price and scarcity have encreased so much that our government have been forced to the expedient of partly relying on a foreign country for the continuation of the navy; and to depend

The scarcity of ship timber has compelled this country to build ships of war in Russia; danger of this expedient.

depend on the dock yards of Ruffia for the bulwark of the Britifh nation, for the defence of its liberties, and of its political exiftence, and this at a time when our crafty and implacable foe has got poffeffion of nearly all the forefts of the reft of Europe, and is making the moft prodigious exertions to out-number our navy.

Should induce the trial of the patent plan, in doing which there is no rifk, as its fufficiency has been proved.

If my plan of fhip-building tends in fo great a degree to diminifh thofe difficulties, and even dangers, as is ftated above, is it not worthy of a trial at leaft, even if fome rifk was run in that trial? but when no rifk is run, when the plan has been proved, the moft fcrupulous economift of the public wealth can ftart no objection to that trial of it in the navy, that the public neceffity for fome expedient to fupply timber for its ufe fo loudly calls for.

No public money required to make experiments, they have been already made at the expence of the owners of the fhip.

We afk no drafts on the public fource to try experiments on the fubject, thefe have been already compleatly made at our own expence, and all we demand is our country to condefcend to reap the fruit of our exertions; if fhe does, we fhall rely on her generofity to recompence us, convinced that fhe will have ample proof that we have deferved it; but fhould this not be the cafe, we will not reft contented with having difcharged our duty, in doing the moft we could to ferve her; which if we fhould be fo happy as to effect, we will never regret our trouble or coft.

The performance of the fhip at fea cannot be mifstated on account of its publicity.

Having thus ftated the claims which the fubject has to public attention, I fhall proceed to relate the performance of the fhip at fea, which, as fhe failed in company with a large convoy both out and home, is a matter of too public a nature to admit any mifstatement I might wifh to make, which God knows is far from my defire.

On the 22d of Auguft, 1804, The patent fhip Economy weighed anchor off Gravefend, with but a fmall cargo aboard, as is ufual for fhips outward bound to her deftination, and fet fail on her voyage to Trinidad and Grenada; and on the 14th October following arrived at Grenada; her performance on this voyage is beft ftated in her Captain's own words, in the following extract from a letter to Wm. Lufhington, Efq. London.

SIR, *Grenada, Oct.* 15th, 1804.

The Captain's letter relative to the voyage out.

I have the pleafure to inform you of the fhip Economy's fafe arrival here yefterday evening. We had a fine paffage, and

and had but one gale of wind: The ship performs as well as it is possible for a ship; is remarkable easy at sea, steers and sails well, and is perfectly tight. In the gale of wind the Epervier man of war sprung her foremast; the Robert Aylward ditto; a brig, Master or brig named Swinger, lost both topmasts and parted convoy in lat. 14. 30 N. Our ship behaved extremely well and never strained a rope yarn.

Ship steers and sails well, is remarkably easy at sea, perfectly tight in a heavy gale, in which other ships suffer much, she meets no accident;

(*Signed*) ALEXANDER SMITH.

From the period of this letter she remained at the West India islands until the 23d of July 1805; being detained there the greatest part of that time by the arrival of the French fleet, which was afterwards chased back to Europe by the gallant and ever to be regretted Lord Nelson; from the 23d. of July, when she sailed for England, to the 29th of Oct. when she cast anchor off Portsmouth on the Mother-bank, she experienced a series of severe weather and violent gales of wind, in which some of the fleet with which she returned foundered, and others were obliged to bear away for America for shelter. The remarkable bad passage home of the Leeward island fleet, of which she was one, is too well known to need much description: all seamen must be sensible that three months tossing on the Atlantic ocean in such hard weather, beating up against contrary winds, to a vessel as deeply laden with sugar as the skrews could compress it into her, must have been a most severe trial, and that if she had a single weak part, or defective principle in her construction, it must have given out in that time: but while most of the other ships of the fleet met with more or less damage both to themselves and their cargoes, she bore through all without the smallest accident, and brought home her sugar perfectly dry and safe; which was not compleatly discharged until Jan. 1806 (on account of her detention at Portsmouth, through contrary winds from whence she she did not get to London before the 27th. of Nov. on which day she hauled into the West India dock,) or this account would have been made public before. A further proof of the stability of her frame work, is her taking the ground with a full cargo on board without any accident, as may be seen more particularly in the following account of her performance home which I received from her Captain.

delayed at the West Indies by the French fleet, returns home, experiences violent gales, and a tedious passage of three months, some ships of the fleet founder through severity of weather. Why this severe trial is a sufficient proof of her stability, when deeply laden, took the ground without injury at Trinadad.

DEAR

DEAR SIR, January 17, 1806.

"IT is with pleasure that I have leisure to inform you of the performance of the patent ship Economy, during the voyage under my command.

Captain's account of the voyage home, In a violent gale the ship performs extremely well, and is a good sea boat: two ships founder in this storm, one abandoned, another large ship rendered unmanageable, and taken in tow, and several others much damaged.

"On the fifth and sixth of September last, latitude 37°, 34 N. we experienced a very heavy gale of wind, with an heavy cross sea, occasioned by the wind shifting to different points of the compass suddenly, and blowing with extreme violence: during the whole of the gale, the Economy behaved as well as I ever experienced a ship to do, and much better than could have been expected for so small a ship; in fine, she is as good a sea boat as ever put keel in salt water. During the gale, two ships, it is supposed, foundered; after the gale one was abandoned as not tenable, should another gale of wind come on: the Prince of Wales, a ship of 300 tons, had every thing washed from her deck: The Princess of Wales, a ship of the same size, broke her rudder, and was left in tow of the Hyæna sloop of war. Several other ships met with considerable damage, which proved undeniably the violence of the wind. Notwithstanding the lumbered state of the Economy, we lost nothing off deck, and I don't think there was a ship, large or small in the fleet, that made better weather: she did not sail so fast coming home as going out, but that is easily accounted for, when we consider she was not coppered, and was out fifteen months on a wooden sheathing, with barnacles as long as your finger on her, and the bottom resembling a rock; and was besides laden as deep as she could stow. She works and steers amazingly well. I would not wish to change her if she had been larger, but being only 200 tons, she is too small both for my interest and the West India trade.

The Economy meets no accident, and is very weatherly; tho' deep laden; has a foul bottom which impedes her sailing, she works and steers well.

"The ship has been perfectly tight all the voyage, although we had a very tempestuous passage, and likewise ran her on shore, *sugar loaded*, under the batteries at Trinadad, to prevent her falling into the hands of the French, as we supposed, where we lay for twenty-four hours, until we discovered that it was Nelson's fleet. In my opinion she is one of the strongest ships in the river Thames of her size.

She remains perfectly tight after the severe passage, though run aground at Trinadad with a full cargo of sugar;

"The new iron slings and other iron work on the yards exceed my most sanguine expectation, I have seen the ship covered with flashes of lightning when at Trinadad, and never experienced

is a very remarkable strong ship,

experienced the least injury from so much iron being about the yards, owing to the precaution which I took of serving the iron work and paying it with pitch, which I think served as a non-conductor. I have a higher opinion of iron work than ever I had, and think the iron rigging in the plan we used to talk about while the ship was building, would answer to admiration, and might be the means of preserving the masts of men of war, when in action, as being less liable to be cut with shot. When I can manage it, I mean to rig the mizen mast of a ship wholly with iron, to give it a trial. When I examine the bottom, I will give you my opinion of the pieces of sheathing steeped in your preparation to prevent the worms from destroying the bottom. The large rollers which you had let in beneath the hawse holes for the cables to work on, were of very great benefit, and I think saved us the labour of two men in weighing anchor, they also prevented the wear of the cables very much, and were greatly liked by the sailors, as making the purchase more lively,

Of the iron slings to her yards, and captain's high opinion of the use of iron work in rigging. State of experiment on part of the sheathing will be attended to, great use of the large rollers for the cables.

Your's very sincerely,

ALEX. SMITH."

The iron slings which Captain Smith mentions, were on a plan of his own, and different from those used in men of war, in not requiring above three or four feet of chain for each yard, and served merely to suspend the yards from the point of the tops; which method greatly saved the wear of the masts, and permitted the yards to work more freely. Iron straps were also used to most of the blocks instead of hemp.

Further account of the iron slings,

The rollers for the cables were about fourteen inches long and eleven in diameter, and worked on iron gudgeons about two inches in diameter, in brass sockets. The rollers which have been hitherto used for this purpose, were generally much too small, seldom exceeding the diameter of the cable; which diminished size both increases the friction and injures the cable, from the smallness of the nip which they occasion; or, in other words, from the acuteness of the angle at which the cable is forced to bend in passing over them.

and of the cable rollers,

In concluding this account I beg leave to mention, that I could, in building another ship, greatly diminish the space necessary for the transverse forms used in my plan, by setting them farther asunder, and forming them of iron, which method

Iron transverse frames may be used in future in this place, to save room.

—and the fore and aft ribs scarped in a more economical method.

method is specified in my patent, and that I could also make a great saving in the timber used in the fore and aft ribs, by a method of scarping them, also within the limits of my specification. Experience has since convinced me of the superiority of both these methods, of which I had some doubt when I built the Economy, or they should have been used in her.

It may seem paradoxical to assert that iron is oftentimes cheaper than wood in ship building, when it can be used: but a plain proof of this exists in the bow of the Economy, of which the three lower breast-hooks are iron of considerable substance, and yet cost less individually than any of the wooden ones above them, though these are of no extraordinary girth, or of much curvature.

The Economy will remain a few weeks at the London Docks for inspection.

The Economy will be a few weeks in the London Docks, where she has now moved, for the inspection of the public, and where all gentlemen who are interested in shipping concerns may see her construction; and those who examined her previous to her sailing, may convince themselves that I have exaggerated nothing, as to the sound state in which she has returned from her tempestuous voyage.

Dear Sir,

Your very humble servant,

J. WHITLEY BOSWELL.

XIX.

Experiments on the Torpedo. By Messrs. HUMBOLDT *and* GAY LUSSAC. *Extracted from a Letter of M. Humboldt to M. Berthollet; dated Rome, 15 Fructidor,* Year 13 (Sept. 2, 1805.)*

THE curious theory with which Volta has enriched the science of natural philosophy, on the subject of electric fish having been received as authentic by many naturalists, renders the phenomenon of the Torpedo worthy of farther investigation. You know, my dear friend, what was our impatience to procure these fish, and will perhaps be surprised that so much time should elapse without having heard from us on the

* Annales de Chimie, Vol. LVI.

subject.

subject. At Genoa, we perceived some; but we were then without our instruments. At Civita Vecchia we fought them in vain. But during our stay at Naples we frequently procured some very large and lively ones. In this letter you will find detailed the experiments made by M. Gay-Lussac and myself on the powers of this fish (*Raja-torpeda* of Linneus). M. de Buch, a German mineralogist, well acquainted with all the branches of physical science, was witness to our proceedings. I send you the results, giving simple facts, unmixed with theoretical speculations. Our experiments were chiefly directed towards the discovery of that state of the torpedo when it was least capable of exerting its power upon the human frame. This power has been generally described as electrical; but the sensation produced by it is materially different from that caused by the discharge of a Leyden phial — Having no other book by us besides the work wherein Aldini* combines the researches of Geoffroy with those of Spallanzani and Galvani, it is not to be expected that we should compare our experiments with those which may have been previously made by other philosophers.

The torpedo found at Genoa and Naples, but not at Civita Vecchia.

The shock of the torpedo feels different from that of electricity.

1. Though the strength of the torpedo is far inferior to that of the gymnotus, it is equally capable of causing painful sensations. A person much accustomed to electric shocks, can hardly sustain that of a lively torpedo of four decimeters (16 inches) in length. The animal acts under water, and it is only when it loses strength that the fluid impedes its action. In this case, M. Gay Lussac observed that the shock is not perceptible till the fish is raised above the surface.

Powers of the torpedo inferior to those of the gymnotus of S. America. Shock of the torpedo more violent than that of electricity. It acts under the water,

2. I observed, when in South America, that the gymnotus gives the most violent shocks, without any exterior movement of the eyes, the head, or the fins: it appeared as tranquil as a person when passing from one idea to another, or from one sensation to another. Not so the torpedo: We observed a convulsive movement of the pectoral fins, each time it gave a shock, which was more or less violent according as the surface was larger or smaller wherein the contact took place.

—and seems to use more effort than the gymnotus.

3. The powers of the torpedo and gymnotus cannot be excited at pleasure, as we should discharge a Leyden phial or a conductor.

Shocks from the torpedo and gymnotus cannot be obtained but by irritating the animal.

* Memoires sur la Torpille, dans l'Essai sur le Galvanisme, Vol. II. p. 61.

conductor. A shock is not always felt on touching an electric fish; it must be irritated before it will give the shock. This action depends on the will of the animal, whose electric powers perhaps, are not kept constantly charged; yet it can recover them with wonderful celerity, as it is capable of giving a long succession of shocks.

The shock obtained by a mere touch with the finger,

4. The shock is felt (the animal being disposed to give it) as well on touching with one finger a single surface of the electric organs, as on applying the two hands to the two surfaces, the upper and under, at once. In both cases it is immaterial whether the person applying his finger or his two hands, be insulated or not.

—but the contact must be direct. Metals seem to be non-conductors of the shock of the torpedo.

5. When an isolated person touches the torpedo with a single finger, it is indispensible that the contact be immediate, as no shock will be felt if a conducting body (of metal for example) be interposed between the finger and the organ of the fish.—For this reason, the animal may be touched with impunity by means of a key, or any other instrument of metal.

6. M. Gay-Lussac having made this important observation, we placed a torpedo on a metal dish, with which the inferior surface of its organs were in contact. The hand which supported this dish experienced no shock, whilst another isolated person irritated the animal, whose convulsive movement of the pectoral fins indicated a most violent emission of the electric fluid.

Experiments which shew that they conduct.

7. When on the contrary, a person held the torpedo in a metal dish in his left hand (as in the preceding experiment), and with his right touched the superior surface of the electric organ, he experienced a smart shock in both arms at the same moment.

8. The same was felt, on placing the fish between two metal plates, whose edges were not in contact with each other, and applying the two hands at once above and below them.

9. But if the edges of the metal plates be suffered to touch each other, no shock will be felt in either arm. The communication between the two surfaces of the organs is, in this case, formed by the plates; and the new connection arising from the contact of the two hands with the plates is without effect.

The organs of the torpedo have no influence on the electrometer.

10. The most sensible electrometer manifested no electrical tension in the organs of the torpedo; in whatever way it was applied, it was not in the least affected; neither, on directing it

it towards the organs, nor in insulating the fish, covering it with a metallic plate, and making a communication between this plate, by means of a conducting thread, and the condenser of Volta, was there any indication (as with the gymnotus) that the animal affected the electric intensity of surrounding bodies.

11. As electric fish, when healthy, exercise their powers as forcibly beneath the water as in the open air, we were led to examine the conducting properties of this fluid. Several persons formed a chain of hands between the superior and inferior surfaces of the organs of the torpedo: the shock was not felt until they had wetted their hands. The action was not intercepted when two persons supported the torpedo with their right hands; and instead of holding each other's left hand, they each plunged a metallic rod into water placed upon an isolated body.

Examination of the conducting powers of water.

12. By substituting flame in lieu of water, the communication was destroyed, until the rods touched each other in the flame.

Flame does not conduct the shock.

13. It must, however, be observed, that in water, as in air, the shock was not perceptible without an immediate contact with the body of the electric fish: the least possible intervention of the water prevented it. This fact is the more remarkable, as it is known that in galvanic experiments, where the frog is immersed in water, it is sufficient to direct the silver forceps towards the muscles to cause a contraction, though a body of water be interposed, equal to one or two millimetres in thickness, or about one-twentieth of an inch.

No shock can be obtained without immediate contact with the fish.

These, my dear friend, are the principal observations which we have made on the torpedo. The experiments, No. 4 and 10, prove that the electric organs of these animals are not susceptible of any intensity or excess of charge. Their action may rather be compared to that of a combination of Leyden phials, than to the conductor of Volta. Without communication no shock could be felt: and having experienced the power of the gymnotus through very dry cords, I imagine, that where I have been affected by this powerful animal without direct contact, it had been occasioned by some deficiency in my insulated state. If the torpedo act by poles, that is by an electric equilibrium which possesses a tendency to replenish itself, experiments 5 and 6 seem to prove that these poles exist near each

Organs of the torpedo not susceptible of any excess of charge.

Doubt whether the shock of the gymnotus can be felt without actual contact with it.

Torpedo supposed to act by an electric equilibrium, the opposite state being very near.

each other, on the same surface of the organ. The shock is felt on merely touching the surface with the finger. A plate interposed between the hand and the organ, (*Exp.* 6,) re-establishes the equilibrium, and the hand which sustains the plate is not affected, because it is placed beyond the current. But if we suppose an heterogeneous number of poles upon each surface of the organ, whence does it arise, that, in covering these surfaces with two metal plates, whose edges do not touch each other, and placing the hands on these plates, the equilibrium should be found in the arms? Why, it may be asked, does not the positive electricity of the inferior surface seek at the moment of explosion the negative electricity of the next or nearest pole, but rather seek it in the superior surface of the electric organ? Perhaps these difficulties may not be insurmountable; yet the theory of these *vital actions* well deserves attentive research. Geoffroy has proved that thornbacks, who give no signs of electricity, are furnished with organs analogous to those of the torpedo. The least injury on the brain of the torpedo destroys its electric powers. The nerves are no doubt concerned chiefly in these phenomena; and the physiologist who should admit the power of vital actions, might with success oppose the theory of the naturalist, who would endeavour to explain all by the contact of the albumino-gelatinous pulp of the nervous laminæ wherewith nature has endowed the organs of the torpedo.

Objections to this notion.

Considerations of theory.

SCIENTIFIC NEWS.

Prizes proposed by the University and Academy of Wilna, in June, 1805.

CLASS OF SCIENCE AND MEDICINE.

First Prize.

To determine whether saccharine secretions take place in other organs besides those affected in diabetes mellitus.

BESIDES the diabetes mellitus of the authors on medicine, are there any other disorders peculiar to man, which, according to experiments well ascertained, produce in different organs a secretion similar to sugar, sufficiently abundant to finally occasion consumption? And what are these disorders?

In a note on this subject it is recommended to examine for saccharine matter, the fluid substance of colliquative sweats; that produced in the *fluxus cœliacus*, and in the pituitous consumption from lungs, which after death are not ulcerated; and the milk of women afflicted with the *gallactirhæa*.

Second Prize.

What are the true characters and the causes of the malady, which although not exclusively appertaining to Poland, is however called the *Plica Polonica?* Are there any means of curing this disease more successful than those hitherto employed? and what are these means? To ascertain the true cause and cure of the Plica Polonica.

Third Prize.

What are the principal maladies of vegetables? And what is the true analogy between them and those of animals? Relative to the disorders of vegetables.

CLASS OF NATURAL PHILOSOPHY AND MATHEMATICS.

Prize.

Suppose a canal, through which a certain quantity of water m, flows in a given number of seconds, through a transverse section of a given depth and breadth, terminated by the two banks: If on this section a dam is constructed, at the top of which an opening is made for the water to pass, of given dimensions; it is demanded according to what law the water, elevated by the obstacle which the dam presents, will be forced to rise not only at the dam, but backwards along the canal. The law is demanded, by which water rises in a canal behind a dam, at the top of which a given opening is made.

Formulæ are required sufficiently general, to be applied not only to the quantity of water m, but to any other $m+x$. Experience not exactly agreeing with the theory, the necessary corrections must be made to the formulæ, and proofs given from facts and observations, shewing how nearly they approach the truth.

CLASS OF MORAL AND POLITICAL SCIENCES.

Prize.

As the sciences of natural philosophy and mathematics make daily advances, and are enriched with new discoveries, it is demanded—

1st.

Qu. why moral ſciences do not make the ſame progreſs as the phyſical? —If they can be farther improved? —What are the bounds to their perfectibility? —What are the beſt methods to attain this point?

1ſt. Why the ſame does not take place in the moral ſciences?

2d. Whether among the different branches of theſe ſciences, there be any capable of a farther degree of perfection? And what theſe are?

3d. To what degree are they of this nature? And what are the limits to their farther improvement?

4th. What are the moſt proper methods to advance the moral ſciences to this boundary of perfection?

It is deſired that the diſcuſſion of this ſubject may be conducted ſo as to preſent reſults, which may contribute to the perfection of that theory of Legiſlation, which is moſt conformable to the nature of man.

Second Prize.

Tenets of Adam Smith and Dr. Queſnay?

To determine (by making an analyſis of political economy) what are the points in which the leading notions of Adam Smith and Doctor Queſnay agree, and in what they differ, or are oppoſite?

This examination muſt neceſſarily produce reſults uſeful to the progreſs of political economy.

Amount of prizes, and laſt days for receiving memoirs.

The prize for each of theſe queſtions is 100 golden ducats of Holland (46*l.* 5*s.*); and the laſt day for the reception of memoirs on medical ſubjects, the 31ſt Auguſt, 1807; and for the others, the ſame day and month in 1806.

Conditions to be obſerved by the Candidates.

To each memoir ſent in muſt be attached a ſeparate and ſealed note, containing the title of the work, and the name and addreſs of the author: This note will only be opened by the Univerſity if the work ſhall obtain the prize.

Memoirs to be written in Latin, French, or Poliſh.

The memoirs muſt be written legibly, in Latin, French, or Poliſh languages. The packet ſhould be addreſſed to the Rector of the Univerſity of Wilna, and addreſſed to one of the bankers of that city, MM. Keyſer or Karner, that it may go free. The Rector will give a receipt to theſe bankers.

The Univerſity ſhall not be obliged to return either the memoirs or the drawings ſent; but the authors will always be permitted to take copies of them.

Conditions relative to property, copy right, &c. of the memoirs.

The Univerſity engages not to print any of the works ſent them, without permiſſion of the authors; but the authors may, at any time, print them if they think proper.

The

The distribution of prizes should take place before the termination of the years in which they are to be determined. The prizes adjudged shall be published in the gazette. Time of distributing prizes.

The author shall receive his prize from the administrative committee of the Imperial University of Wilna, either in person or by deputy. The prize will be at his option, either a gold medal or 100 golden ducats of Holland.

The Professors and honorary members of the University of Wilna, cannot be candidates for the prizes. Professors of Wilna not to be candidates.

Revived Precipitates from alkaline Solutions of metallic Oxides.

M. Klaproth, a little before his decease, discovered that the solution of the metallic oxides in the alkalis, are as easily precipitated in their metallic state, by the other metals soluble in the same alkalis, as are the acid solutions of these metals by phosphorus: He has made a very ingenious application of this process to the analysis of tin ores, according to the method which is described in his *Beitraege*: In this operation tungstein is separated from tungstate of ammonia, by the addition of zinc, in the form of black flakes. Alkaline solutions of metallic oxides precipitated, in the metallic state, by other metals.

Experiments on falling Bodies, by M. Benzenberg.

M. Benzenberg, professor of physic and astronomy at Dusseldorp, published, some months ago, twenty-eight experiments made with balls well turned and polished, which were made to fall from a height of 262 French feet: At a medium they produced five lines of deviation towards the east, according to the determination of the plumb-line, and the theory gives four lines six tenths. These experiments were made in the coal-mines of Schebusch; they are an additional proof, if it were necessary, of the rotatory movement of the earth, of which no one now doubts. The last experiments, made at Bologna by M. Guglielmini, gave nearly the same results. A falling body deviates four lines six tenths east, after passing through 262 feet.

Geography.

Great pains are taking in the construction of an accurate map of Holland: The same precautions have been used in this business as in the measurement of the degree of the meridian. M. de Zach has published in his Journal the chart of the triangles which have been completed: They are joined to Map of Holland.

those

those which M. Delambre made for the great meridian; and the distance from Dunkirk to Montreal has been taken for the first base. When the triangles are finished, a base will be measured towards the north, to serve for the verification of the work. The Batavian republic have entrusted the direction of this map to Colonel Krayenhoff.

Chart of the White Sea, by General Kutuzoff.

Some months ago there appeared at Petersburgh a very fine hydrographical chart of the White Sea, of which General Kautouzoff is the author: Many naval officers have worked under his direction for four years, in collecting the materials necessary to compose this chart. The coasts of the White Sea, of its gulphs, and of a part of the Northern Ocean, have been laid down trigonometrically. The depths have been carefully sounded; and six of the principal points of the coast have been determined by astronomical observations.

M. Lartigue's map of America in relief.

M. Lartigue having been engaged for thirty years in constructing, at the marine depot (of Paris), a large and beautiful map of America in relief, has at length completed it. It is said that the mountains, and the islands, and the tints of the sea, are all exhibited in a manner most capable of interesting those who make geography their study.

Expedition of Capt. Lewis up the Missouri.

Several months ago, Captain Lewis, in America, undertook to ascend the river Missouri, in search of a passage to the South Sea. Very interesting intelligence may be soon expected from this expedition.

Survey of France.

The work of the government survey, or *cadastre*, of France, has proceeded with activity; 2000 persons are employed in it in the 108 departments.

Effect of Heat on Magnetism

Magnetism destroyed at 700° of heat.

M. Coulomb has published an interesting memoir on the effect of heat on magnetism. At 200 degrees of heat, two-fifths of it are destroyed, and the whole at 700 degrees.

A Secret Lock of Ten Thousand Combinations.

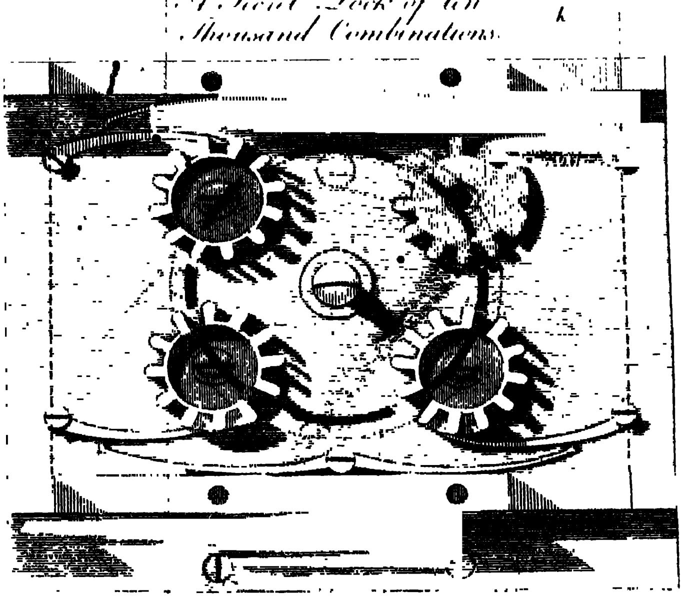


Fig 1

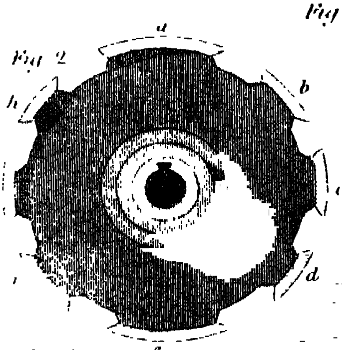


Fig 2

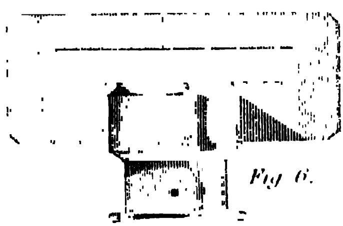


Fig 6.

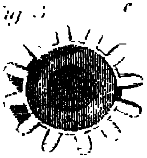


Fig 3

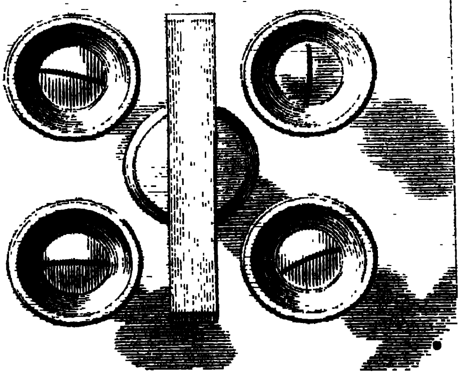


Fig 4.

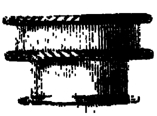
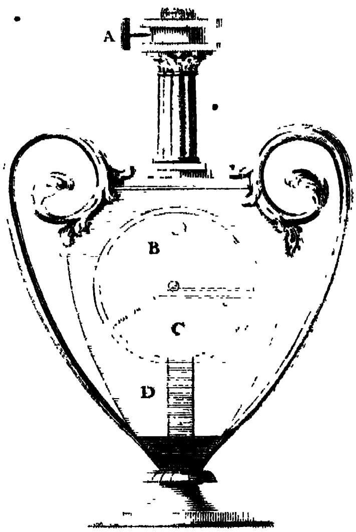


Fig 5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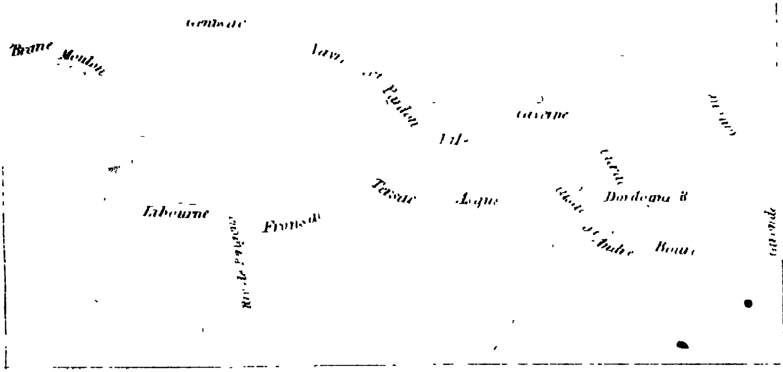
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JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

MARCH, 1806.

ARTICLE I.

Experiments on the Temperature of Water surrounded by freezing Mixtures. In a Letter from JOHN GOUGH, *Esq.*

To Mr. NICHOLSON.

SIR, *Middleshaw, Jan.* 29, 1806.

MANY philosophers have turned their attention to the dilatation observable in water when cooled below 40 or 41 degrees of Fahrenheit's scale, and also to the no less singular fact of water retaining its fluidity for a considerable time when exposed to a freezing mixture, without being agitated. But one circumstance, relating to the latter phenomenon, appears to have escaped the notice of them all; which in all probability will prove of some importance to both enquiries.

Expansion of water in cooling below 41 deg.

We know from common experience, that when a hotter and colder body come into contact, the former will lose and the latter acquire heat, until they arrive at an equality of temperature. The frequent opportunities every one has of making this observation have authorised it to pass for a general rule; hence it has been concluded, that water in a state of rest may be cooled many degrees below the freezing point,

Explained by a developement of the conjecture that ice may be formed in minute crystals at that temperature.

 and

and ſtill remain fluid. For my part, I adopted the maxim without heſitation, until the peruſal of Dr. Hope's paper, given in the ſupplement to your laſt volume, led me to reaſon in the following manner on the ſubject.

When water is expoſed to a freezing mixture, thoſe particles of it which are in contact with the ſides of the veſſel, are ſoon reduced to a temperature lower than the point of congelation; in conſequence of this, they are probably converted into minute icicles, which impart a quantity of caloric at the moment of their formation to the ſurrounding water, thereby preventing its temperature from ſinking below 32°. Theſe inviſible bodies afterwards begin to aſcend ſlowly on account of the diminution in their ſpecific gravity; and while they riſe towards the ſurface of the water, other particles will approach the ſides of the veſſel in ſucceſſion, and undergo a ſimilar transformation. This proceſs would evidently increaſe the volume of the water without reducing its temperature, ſuppoſing it to be ice-cold at the commencement of the experiment; for the hypotheſis reſts on the ſuppoſition that water freezes as ſoon as it is cooled below the 32nd degree of Fahrenheit's ſcale. This gradual increaſe of bulk will explain the appearances deſcribed by my friend Mr. Dalton, who found that thermometers filled with water continued to riſe when expoſed to freezing mixtures, until the encloſed water congealed ſuddenly, and frequently burſt his inſtruments. The reaſon why agitation accelerated the congelation of water thus circumſtanced appears to be this: When the inviſible icicles become very numerous, the leaſt motion carries them in crowds againſt the ſides of the veſſels; where the ſmall quantity of water contained amongſt them cryſtallizes immediately, and cements the whole into a film adhering to the inſide of the cup. This theory, or hypotheſis, call it whatever you think proper, evidently requires water to be what it really is, namely, a bad conductor of heat; and after forming it, I proceeded to examine the merits of it experimentally, in the following manner.

Experiment. Water at 32° was cooled by a ſurrounding mixture, and became congealed at the ſides by ſtirring, having never ſunk below 32°.

Experiment 1ſt. A ſmall thermometer was ſuſpended at the lower end of a wire, which could be moved in a vertical direction, through a hole in a horizontal bar of wood, fixed over a table for the purpoſe; a veſſel, containing a freezing mixture, of the temperature of 21°, was next placed with

5 its

its centre under the wire; and the bowl of a wine glafs, filled with two ounces of ice-cold water, being then properly placed in the mixture, the thermometer which ftood at 32°, was immediately let down into the water, where it remained ftationary for the fpace of feven minutes. A wire, cooled to the freezing point was now introduced into the glafs, and the water agitated with it; upon which a thick coating of ice formed on the infide of the veffel; but no marks of congelation were obfervable on the wire or thermometer.

Exp. 2. The fame apparatus being ufed, with a mixture, having the low temperature of 6°, the glafs was filled with water of 58°, in which the thermometer fell to 32° in $7\frac{3}{4}$ minutes, by a ftop watch; at which point it remained ftationary five minutes longer. The glafs was then taken out of the mixture, and the water being agitated, lined the upper part of it for about two-thirds of its depth from the brim, with a porous covering of ice, but the remaining part of it was free from all incruftation.

Exp. 2. Water at 58° was cooled by an intenfe freezing mixture. It was brought to ftationary 32° and funk no lower. When taken out and fhaken the top froze.

I will venture to infer from the two preceding paragraphs, that we have all been under a miftake in concluding that water may be cooled when at reft many degrees below 32° of Fahrenheit, without congealing; at the fame time we are certain, that it will preferve its fluidity, when judicioufly expofed to great degrees of cold, and dilate at the fame time, as Mr. Dalton has proved. Now as the heat never falls below 32° in thefe experiments, the expanfion of the water in Mr. Dalton's thermometers, placed in a freezing mixture, cannot be afcribed to a lofs of temperature, but muft be owing to fome other caufe, probably to that which has been affigned above. As for agitation, the firft experiment feems to fhew its office to confift in bringing the water, crowded with minute icicles, into contact with parts of the veffel much colder than itfelf, where it is concreted into ice.

Hence water cannot be cooled and remain fluid at temperatures below 32°, as generally fuppofed.

The water thermometer muft expand in cooling by fome other caufe.

Exp. 3. To examine this part of the fubject with more care, I formed a cup of caoutchouc, the capacity of which for caloric greatly exceeds that of glafs; or, I believe, that of moft other fubftances. Two ounces of water, a little warmer than melting fnow being poured into this cup, it was placed in a mixture of the temperature of 15°, where it remained eight minutes without giving the leaft indication of a tendency to freeze. The cup was now removed from the

Exp. 3. Repetition of Exp. 2 very ftrikingly in a cup of caoutchouc.

mixture, and gently ſhaken; upon which long icicles formed in an inſtant, projecting into the water in all directions, from the caoutchouc to which they adhered. This experiment, I have no doubt, might be made a very beautiful one by a dexterous operator, who is in the habit of exhibiting natural appearances to public aſſemblies.

After diſcovering that water will dilate without any change of temperature from warm to colder, at 32°, I began to imagine that the whole variation of expanſion under 41°, might be explained on the ſame principle, becauſe I believe all the experiments relating to the ſubject, have been made in a cooling medium, not warmer than melting ſnow.

Water expands by cooling between 41 and 32 deg. or begins to cryſtalize at the upper term.

In order to try the merits of this opinion, with an inſtrument larger than a common thermometer, I filled a four-ounce phial with water, and fixed an open tube into it, by means of a perforated cork and cement; but this apparatus proved my ſuſpicion to be falſe. For the place of the water being marked on the tube when the temperature was 41°, my bulky thermometer roſe immediately upon being plunged into water of 34°. This fact proves, that water expands by a loſs of temperature between 41° and 32°; or elſe, that this fluid begins to cryſtalize at the upper term; in conſequence of which the lower term, or 32°, is not, properly ſpeaking, the commencement of congelation, but the point at which the cryſtals of water begin to concrete into maſſes by aggregation.

I remain, &c.

JOHN GOUGH

II.

Account of the Art and Inſtruments uſed for boring and blaſting Rocks; with Improvements. In a Letter from G. C.

To Mr. NICHOLSON.

SIR, *Briſtol, Jan.* 21, 1806.

Improvements in blaſting mentioned.

BY way of appendix to Mr. Cloſe's remarks on the uſe of ſand in ſtemming mines in hard rocks, and his uſeful improvement of the pricker, by making it of copper inſtead of iron, allow me to add two other improvements in the art of blaſting ſtone,

ftone, which my own experience has proved to diminifh confiderably the expence of gunpowder, while one of them, at the fame time, removes all danger from imperfect priming.

Defcription of the art as practifed in the lime ftone rocks in Somerfetfhire.

I fhall alfo, with your permiffion, as many of your correfpondents muft neceffarily be ignorant of the conftruction of the tools, give you a defcription of thofe now in ufe at the village of Shipham, in Somerfetfhire, a village wholly compofed of men, women and children, who mine after lead ore, calamine, and ochre, chiefly in a lime-ftone rock; a numerous band of fome of the ftouteft beings in England.

The iron pricker ufed without bad effect with fpar for ftemming.

Thefe men ftill ufe the iron pricker, becaufe an accident feldom or ever happens to them; owing, I believe, in a great meafure to their ftemming with fpar, and their habit of turning and loofening the inftrument at every half inch they fill.

The tools they ufe are thefe, *Plate* V.

Tools and implements defcribed. A hole eighteen or twenty inches deep and half an inch in diameter is cut by repeated ftrokes of a chiffel, the edge of which lies in the diameter of the hole and is fhifted round between ftroke and ftroke.

A. A round bar of iron, bevilled off at one end, of 18 inches long, and of the diameter of half an inch.

B. A ditto, of 24 inches, to follow when the hole in the ftone is about 12 inches deep.

C. A rod, with a loop for the finger, 25 inches long; at the bottom of which is a round flat plate of iron to draw out the pounded ftone occafionally.

D. A pricker, 24 inches long, with a loop alfo, ufed to preferve a paffage to infert the priming ftraw, while the hole is rammed or ftemmed with E, the iron rammer, 20 inches long, and which, fix inches or more from the end, is formed into a conical groove, very open at bottom, in order to enable the miner to ram round the pricker, and alfo that by its fharpnefs at the end it may the eafier break to duft the pieces of fpar dropped in as faft as wanted.

F. A hammer with a handle and ftrap, about five inches long; the iron head weighing about four or five pounds, according to the ftrength of the operator; for fome have them of fix or feven.

The work is performed wet, and the chipped ftone fcooped out with an inftrument.

They alfo have by them a bottle of water, to pour occafionally into the hole, for the wetter it is the fafter the work goes on.

At every ftroke of the hammer, the miner turns his chiffel, by which means he works the bottom of the mine in a regular circle, and is enabled to keep his perforation true.

When arrived at the depth of 18 or 19 inches, he cleans,

and,

The charge of powder is an ounce (which is too much). An iron wire called the pricker is put down in the hole close to the side, and small pieces of spar are dropped in, which are slightly rammed and afterwards more firmly. The whole being full, the pricker is drawn, and a wheat straw filled with gunpowder is put down in place of the pricker.

and, as well as he can, dries his mine; then inserts his charge of gunpowder, often amounting to the unnecessary quantity of an ounce, and dropping the pricker to the bottom, with its side touching the side of the mine, he begins by dropping into it some lumps of spar; and after he has filled up about an inch, begins pounding it round the pricker with his rammer and hammer; tapping gently at first, but soon beginning to ram very hard, all the while frequently turning and loosening the circular pricker.

When the hole is quite filled, he draws it, by giving some gentle strokes on the chissel that he has now passed through the loop to draw it with.

He then takes the upper joint of a wheat straw, the smallest he can get, and having stopped the fine end with clay, if it has no knot; he afterwards places the other end, cut off very bevel and sharp, between his second and third finger of the left hand, close to where the fingers join the palm, forming his hand into a kind of bason, to keep off the wind, and drawing the open end of the straw so low between the fingers that he can but just prevent it from dropping on the ground; when pouring a small quantity of gunpowder on the orifice, and tapping with his other hand on the straw below, to shake it, it speedily is filled.

This straw must be 19 inches long for a hole of 18 at least, and a little shaved away at the bottom, but not cut open of course.

Fire is given by a piece of touch-wood,

When thrust down to the powder the train is compleat, and our operator lastly lights a piece of touch-wood, and places it so that when all on fire, it shall communicate to the train; after which he withdraws to a place out of the line of explosion, and waits its effect.

—which occasions loss of time by its failure and danger, when too rapid.

And here in blowing a well, I found that much time was lost; for not only does the wind occasionally blow away the touch-wood before it is all inflamed, but frequently the damp extinguishes it. I also found there was danger to the workman if it went off too soon, which the wind sometimes occasions, or his companion is too slow in haling him up; and we likewise found that when they worked by the day, and we found powder, they used an immoderate quantity.

Improvements. If a cork be thrust down the hole or well pre-

To remedy these two great evils, I pursued the following plan: the first of which was suggested to me by an ingenious neighbour and both had the desired effect.

The

The first experiment I tried was upon a single block of limestone, of about two ton weight. I charged the mine with only the common charge of a musket, as at K, over which I drove a cork, as at H, leaving one inch, or thereabout, as at I, over which I rammed spar, as at G, up to the surface of the rock.

vious to stemming, so as to leave one inch of windage, less powder will be requisite.

I then made a slit in my straw train, as at L, and passed through it, as through a loop, a cut of the German ash-tree fungus; but not liking that, as endangering the loss of the priming powder, I cut the slit in the fungus, as at N, passing the straw through the slit, and cutting a small notch on one side of the straw, as at O. When it was slid down to it, being elastic, it closed there, and filled the notch.

The German fungus or amadou is steadier and more certain than touch-wood. The straw may be thrust through a hole in this fungus,

This match burns slow but sure, and no wind can extinguish it. A great advantage, as I have frequently witnessed, in making the new and beautiful towing path on both sides the Avon, from Bristol. One hundred men lose from ten minutes to twenty and more while getting out of the way during the blowing of a mine near the spot they were levelling, and all owing to the slow burning of the touch-wood match, or the wind blowing it aside.

—which burns slowly, but with certainty, and is not blown out by the wind.

This German match is, I fancy, pretty well known; it is merely the fungus of the ash-tree, macerated and hammered until it becomes as flexible as a piece of buff leather, and has been called the German match, I believe, from its general use on the upper Rhine, where, by its means, habitual smokers of tobacco can light their pipes in the open air, whatever may be the weather; and as a piece which scarcely weighs four grains is sufficient to light without danger, the largest mines, while the article is by no means dear, and always safe, inextinguishable, and regular in its burning, nothing can be more useful to the practical miner.

Account of the fungus.

With respect to sand (which I see recommended in a Dublin paper of only last week, as a new discovery in stemming), it will not always succeed, especially in those great mines of the Clifton blasters, where, often 15 or 16lb. of powder are used at a time; but I should think if stopped with a stiff clay, it would greatly encrease the resistance, especially if sufficient windage was left over the powder.

Stemming with loose sand will not (it is thought) succeed in mines upon a large scale.

The experiment I first tried, as described above, on that plan, tore to pieces, and threw four pieces of my rock to a great

The author's experiments were fully successful.

great height, ſhaking and leaving fit for loading, a good cart load in all; while, but for a wall at hand, my Shipham miner, as uſual, deſpiſing novelties, would probably have been wounded, having been with difficulty perſuaded to take that cover at four yards diſtance.

Thus, Sir, I have ſtated what I take to be improvements in this valuable art, and if they afford you or your readers any gratification, I ſhall not regret the trouble of putting them on paper, being always, Sir,

Your grateful reader,

G. C.

III.

Deſcription of a new Parallel Rule, exempt from lateral Deviation; invented by Mr. J. W. Boswell; *with an Account of the Imperfections of thoſe already made for the ſame Purpoſe.*

To Mr. NICHOLSON.

Dear Sir,

Inconveniencies from the lateral deviation of the common parallel rule.

THE common parallel rule of four pieces has been long found inconvenient, on account of the lateral deviation of the moving piece, which cauſes a neceſſity of ſhifting the poſition of the whole rule frequently, when many parallel lines are to be drawn; that, beſides the loſs of time which it occaſions, tends alſo to produce error in the parallelifm of theſe lines. For this reaſon it is in no reſpect ſuperior to the more ſimple apparatus for the ſame purpoſe, formed by a triangular plane of wood or metal, moved along a common rule; and as this latter is more ſteady, and ſerves for other purpoſes in drawing, it is preferred by ſeveral.

It is not ſuperior to the triangular plane and rule.

Parallel rules as yet contrived to operate without ſide deviation are ſubject to inaccuracy,

Many inſtruments have been contrived to draw parallel lines without being ſubject to the imperfections here ſtated; but all, that I know of, are more or leſs deficient in correctneſs, from requiring an exactneſs in their formation hardly attainable, or from extreme tendency to have this perfection deranged when attained.

The parallel rule with ſliding joints difficult to make exact, and

The parallel rule with croſſing connectors, and two ſliding joints, is ſubject to both the above inconveniences. The leaſt play

play in the slides, or deviation in the grooves in which they are to move, must alter the parallelism of the lines drawn by it; and however exact it may be at first, the natural wear attendant on its use must demonstrably produce these imperfections; to which may be added, that the nicety of workmanship which it requires, and its complicated form, must of course render it expensive. becomes inaccurate from wear.

The instrument formed by a rule supported by two small wheels fixed to the same axis, which axis is placed so as to be parallel to the edge of the rule, is liable to be imperfect, from any difference in the diameters of the two wheels, or slight inaccuracy in the position of the axis. That moving upon two wheels fixed to one axis, has the same defects, and is liable to inaccuracy from the unevenness of the paper.

This rule is also very liable to slip on the paper, and is rendered incorrect in its effects by any unevenness in the surface over which it is moved.

The parallel rule, mentioned in your ninth volume, page 212, requires an exact proportion in the length of each of its parts; and as these are all of different measures, would be liable to error in the first formation, on this account; and however exactly made, would, after a little wear, soon deviate, on account of the play which this would produce in the joints; the connectors also between the two rules, passing from different extremities, and leaving long spaces beyond the points of support, would thereby occasion any play in the joints to produce a greater deviation from parallelism in the lines drawn. That formed by several unequal joints difficult to make exact, and liable to become very incorrect in wear from its long projections beyond the points of support.

The apparatus for producing parallel lines formed by the drawing board and normal square, can hardly with propriety be classed among the instruments here treated of; whatever its accuracy may be, its cumbrous form, and the time required for fastening the paper to it, render it for many purposes very inconvenient. The drawing board and normal square is cumbrous, and wastes time.

These considerations induced me, about the time when the account of the parallel rule, given in your ninth volume was published, to consider how a parallel rule might be constructed not liable to side deviation, and as free as possible from the defects of the others above stated. The instrument which then occurred to me as the best calculated for this purpose, I shall now describe; and as I have often examined it since, if it possessed any material defect, it is probable it would have become manifest before this; in which case I should not have brought it forward to public notice.

My

Description of Mr. Boswell's parallel rule to prevent lateral deviation.

My instrument for drawing parallel lines without side deviation, is formed of three rulers, laid parallel to each other, connected by two pair of moveable pieces, all of equal length, and parallel to each other; these pieces, where they meet on the middle rule, have their extremities formed into portions of toothed wheels, which lock into each other, as may be seen in the figure: the effect of these segments of wheels thus acting in each other, is, that all the lateral motion is transferred to the middle rule, while the external rules move only in an opposite and parallel direction.

The contrivance to prevent lateral deviation cannot affect its accuracy. Its support at each end makes it steady. It is easily made exact.

This instrument will not be liable to the incorrectness of those before described, for the following reasons: 1st. The toothed segments being in no way concerned in producing the parallelism of the instrument, its accuracy of parallelism cannot be at all affected by any trifling incorrectness of formation in their parts. 2nd. All the connecting pieces being of equal length, can be formed with more certain accuracy. 3d. The connecting pieces passing from the same extremities of the external rules, give them a steady support. For these reasons, in my opinion, it possesses all the steadiness and facility of formation of the common parallel rule, while it effectually prevents the side deviation, to which the latter is liable.

It might be made with but one pair of toothed segments, but two pair make it look more uniform.

It is not absolutely necessary to have more than one pair of the connecting pieces made with toothed segments; but as these segments are easily formed in the clock makers engine for cutting teeth in wheels, it can add little to the expence to make the two pair in this manner, as shewn in the figure, and will make the instrument look more uniform.

The middle rule should be made thinner than the other to prevent friction.

The middle rule should also be made a little thinner than the others, to prevent friction on the paper in its lateral movement when in use.

Novelty of the instrument consists in its toothed segments.

In the description of this instrument, it will be observed, that the novelty of it consists in the application of the toothed segments of the wheels to the use mentioned; which I cannot find has ever been before used for this purpose; and I think it highly probable it has not, as, besides its not being known to gentlemen, whom I have consulted on this head, most likely to be acquainted with such matters, the simplicity of the contrivance would probably have brought it into extensive use, if it had been ever known at any former period.

Reasons for supposing this invention to be new.

I mention this only to shew that, before I claim the priority of invention, I have taken some pains to investigate my pretensions;

tenfions; which I think is incumbent on every man to do on fuch occafions: for, however fair the claim may be of invention, if a thing is well known to have been before done, it at leaft produces an aukward fenfation to the claimer; for which reafon, thofe who accufe others of doing this, fhould be the more cautious, that their accufation is fair in all its parts; for oftentimes an external refemblance may fubfift between two contrivances, as between my inftrument and the triple parallel ruler, and yet a fmall addition render their effects effentially different; thus the triple parallel ruler admits of fide deviation, while my parallel ruler effectually prevents it.

A fmall addition to an inftrument fometimes renders its effects effentially different.

My motive for publifhing the account of this inftrument is principally becaufe I think it a duty incumbent on every man, who has contrived any thing that may be of ufe to the world, to make it known as extenfively as poffible, which it certainly will be by appearing in your Journal.

The inftrument from which the figure was drawn was made according to my directions, by Mr. Banks, inftrument maker, No. 441 in the Strand, and anfwers the purpofe perfectly well; of courfe any gentlemen who defire to ufe parallel rulers of this kind, may have them accurately made at the fame place.

This inftrument made by Mr. Banks, 441, Strand.

I requeft the favour of your permitting the infertion, at the end of this communication, of the indication of fome typographical errors, made in my paper relative to the performance at fea of the fhip Economy, in your laft number; and which I am the more anxious to have rectified, as fome of them entirely alter the fenfe of the paffages where they occur.

Typographical errors in Mr. Bofwell's laft paper relative to the fhip Economy.

Page 175, line 2, erafe *it* before *could*; line 6, transfer the bracket to before *when* in the next line; line 8, for *is* read *are*; erafe the comma after *is*; and transfer the bracket to after *proof*; line 9, 10, for *direction* read *directors*; page 176, line 15, for *fource* read *purfe*; line 21, erafe *not* before *reft*; page 179, line 22, for *point* read *front*; line 37, for *forms* read *frames*; page 180, line 3, for *fcarping* read *fcarfing*.

Some errors of the prefs are alfo apparent in the fide notes, but I fhall not trouble you by pointing them out, as they can be rectified by the meaning of the paffages to which they are added. I am, Dear Sir,

Your very refpectful humble fervant,

J. W. BOSWELL.

Reference

Reference to the Figure, Plate V, Fig. 2.

A A, A A. The external parallel rules, B B the central rule, C D, C D the connecting pieces, D D the segments of toothed wheels in which the connecting pieces terminate, which by their action on each other prevent side deviation in A A, A A.

IV.

Letter from an ENQUIRER, *on the Waste of Fish asserted to be made on the Scottish Coast. In Reply to A. L.*

To Mr. NICHOLSON,

SIR, *London, Feb.* 7, 1806.

Proper spirit of inquiry and publication.

I AGREE most cordially with your Correspondent A. L. of Aberdeen, in page 168, with regard to the accuracy of important information when communicated to the public, and that when doubts exist, it should be given with so much modesty and diffidence, as to shew that the communicator is not certain of his subject. Of the statement I made respecting those instances of wasteful negligence in some fisheries of the north of Scotland, I am not the first; the respectable author of the statistical account of the parish of Peterhead, the Rev. Dr. Moir, has asserted the same, limited to that parish that I did. In the 16th Vol. p. 550 of that work, he says, "turbot (I believe the holybut of the London market) is now caught frequently, and in great perfection. Thirty years ago they were seldom used here, *frequently cast into the dunghill*, or *left to waste on the sea beach*, they at present sell from four pence to one shilling each, and are rising every day in price;" in the preceding page of that volume we are informed, that "the greatest part of the *cods' sounds*, in this parish, are *permitted to remain and rot on the sea beach*, or, *are cast into the dunghill*, though the use and value of them as an article of food and delicacy at table have been known here for many years," and yet in the following paragraph the Doctor tells his readers, "that the crews of the ships have been sent from this town to Barryhead, *to preserve the sounds*, tongues, and palates of the cod caught *there*, and the owners have always

Scotch fisheries.

found

found a ready market for them!" for myfelf, Sir, before I even hinted through your Journal, at thefe ftrong affirmations, I made it my bufinefs to enquire of fome friends at Aberdeen, of the truth or falfehood of fuch affertions; deeming it then, as that gentleman does at prefent, an improbable ftatement; under thefe circumftances I cannot confider the communication you did me the favour to infert, as militating either againft your correfpondents rule of examination, or my own habitual fcepticifm:—that gentleman, in recommending accuracy of ftatement, ought not to have forgotten it himfelf; he will eafily fee that *Aberdeen* is not mentioned by me as being at all concerned in this wafte of fuftenance. My little note to you has rouzed the attention of A. L.—Is it wandering too much into

"*the fairy regions of romance,*"

to hope that the fubject may obtain ftill farther notice? and continue to do fo until it be made productive of *all the advantages it is capable?* in that cafe, fuppofing defective information in my firft notice of it (and I prefume A. L. will allow I had fome authority for my opinion, and that he himfelf has not been completely accurate) my errors will be eventually attended with good. Your correfpondent well knows, that the affertion of Dr. Johnfon about the fcarcity of trees in Scotland, has had the happieft effects. How far that gentleman's queftion about the vend at the towns I mentioned, may be anfwered in the affirmative or otherwife, I have not yet fufficiently informed myfelf; but certainly under the circumftances I conceive to be true, thofe markets would be preferable to fuch wafte. The men employed may look with confidence for a fpeedy fale; and, if I am not very much miftaken, thefe towns are fupplied from the Yorkfhire boats, the wind therefore which brings the one fet of boats, would impede, if not totally hinder the other. Far am I from wifhing to throw any obftacles in the way of fo excellent a plan, as that for a fociety for exporting white fifh from Aberdeen, but where would your correfpondent fend them to? "*is it not to be fuppofed, that fifhers of the places neareft to fuch towns could greatly underfell them?*" this queftion is not a greater difficulty in the fcheme fuggefted by me of bringing the fifh to Leith, Berwick, or Newcaftle, than it is to that of A. L. The fact is, that by giving that queftion weight, competition, in every bufinefs

Good effects of inquiry and public remark

business would be undone; for my own part, I do not think it any objection to either of our schemes; nay, our plans considered seriously, are nearly alike. I suggested, rather I believe in the form of a query than otherwise, the propriety of bringing fish where I know a demand exists; but that gentleman opposes my suggestion with the above question, and then proposes a plan on a similar, but more comprehensive scale! Allow me merely to correct at present one more error A. L. has, *unintentionally, I am certain,* committed in the last paragraph of his vindication of the Arbroath fishers (of whose methods, and *probable* waste, I hope soon to obtain a *correct* account,) It is *not* the case even for the most part in *every* large fishing town, that the fishermen " retain the bodies of the crabs, and sell the large claws only. A list of some of the towns in which that custom prevails, would, doubtless oblige many of your readers. If I am correct in my opinion of A. L. all *his* attempts are for the spread of useful knowledge; his candour will suggest the propriety of viewing mine also in a favourable light.

I am, Sir,

Your's and A. L's,

Friend and Servant,

AN ENQUIRER.

Notice of a Publication of Importance intended by the Literary and Antiquarian Society of Perth.

SIR,

IT is with great pleasure I inform you that the very respectable Literary and Antiquarian Society of Perth, intend giving a selection of their valuable papers to the public; it is much to be lamented that they have delayed doing this so long, as many of their manuscripts throw a very extensive light on the antiquities of that part of the island.

I am, Sir,

Your's truly,

N. L.

To Mr. Nicholson,

Newcastle-upon-Tyne, Jan. 28, 1806.

Letter concerning a Library established at Aberdeen. From a TRAVELLER.

SIR, *York Hotel, Bridge Street, Blackfriars.*

I AM extremely glad to find, that there is a subscription library established in Aberdeen. I am astonished, however, to be informed from Mr. Crombie's paper, that none of the very learned Professors in that part of the country are engaged in the undertaking! perhaps if the subscription was raised to one guinea per annum, much more good might be effected, and those gentlemen would not then scruple to join themselves; their freedom as to pecuniary motives is well known. I hope for the sake of the general diffusion of knowledge, to find myself equally mistaken with regard to Banff, Peterhead, and Inverness. The society at Aberdeen, though young, seems to be conducted with great liberality, as appears evident from their offer of assistance to the places above mentioned, or any other that may be now forming rules; is it too much for a friend to the spread of useful knowledge to suggest to them an extension of the benefits of their association, to those gentlemen who are members of similar societies, whilst in Aberdeen, on condition of a return of such civilities, should any of their members be where such libraries are? a rule of this kind you Sir, have mentioned with applause in a former number,

Your's, &c.

A TRAVELLER.

February 7, 1806.

V.

A Chemical and Medical Examination of the Gizzards of White Fowls compared with Gelatine, together with an Exposition of the Characteristics of the latter when oxigenated, By M. BOUILLON LAGRANGE *.

IT has long been understood, that the gizzards of white poultry possesses certain medicinal qualities. The use made of it by many physicians may justify some reliance upon the Gizzards of fowls medically used.

* *Annales de Chimie*, Vol. LV.

virtues

virtues attributed to it; but no one, I believe, has hitherto thought of analyfing this fubftance.

It occurred to me, that it would be ufeful to the art of healing, were a few chemical facts added to the knowledge already poffeffed of the medical ufes of gizzard, particularly after reading in the "*Journal d'Economie Rurale and Domeftique, ou Bibliothèque des Propriétaires ruraux, Pluvoife, an* 12;" a letter, wherein is announced the fuccefs obtained by its ufe in agues. As this letter contains the details of the preparation, and adminiftration of this remedy, I fhall tranfcribe it at length.

"*Amiens, 25 Frimaire.*

Letter refpecting it. Recommended as a febrifuge by the French government.

"YOU mention animal gelatine as a febrifuge, I will inform you of a more fimple and lefs expenfive remedy. I know not by what fatality this great fpecific has been neglected, notwithftanding it was publifhed by government full forty years ago, and in fpite of its efficacy, of which I have had long experience; for, of about a thoufand cafes, in which during that period, I have adopted its ufe, I can atteft the cure of eight-tenths.

"I have refided at Montpellier during fifty-fix years; the climate of the place and its environs is mild and falubrious; but the inhabitants along the coaft are fubject to agues, on account of the vicinity of the Mediterranean, and of ftagnant pools, M. de St. Prieft, intendant of this province, publifhed the order of government relative to the remedy above alluded to.

Prefcription.

"*Remedy.*—This remedy confifts of the gizzard of fowls, dried and pulverized.

The gizzards are wafhed, dried, and pulverized.

"*Preparation.*—Take the gizzard of white poultry, as fowls, turkies, &c. (I never made ufe of thofe of black fowls, as pigeons, ducks, &c,) open them, and clear away the gravel they contain; having flightly wafhed them, let them be put on a ftring and hanged in the fun, or up a chimney to dry, after which they muft be reduced to powder, fifted, and kept in a bottle clofely corked.

"*Dofe.*—The dofe is about a drachm for adults, and from half a dram to a fcruple for children.

The dofe is one drachm taken in wine.

"*Mode of taking.*—Mix the proper quantity of the powder in a glafs or half-glafs of good old white wine, and let the patient

patient swallow it about half an hour before the fit comes on, or on the appearance of the precursory symptoms of the fever. This being thrice repeated, it rarely happens that the disorder returns.

"*Regimen.*—A wholesome regimen is all that is necessary during the administration of this medicine, but the patient should carefully avoid exposure to moisture or cold, particularly in the feet."

The foregoing details lead us naturally to the following observation:

Should this substance be considered as gelatine, and possessing the same property of being a febrifuge, as stated by M. Seguin; or should we rather acknowledge it to be possessed of those particular virtues which have been attributed to it by several emient physicians? M. Pia, an old apothecary of Paris, assured me, that full thirty or forty years ago, the powdered gizzard of poultry was recommended in all obstructions of the urinary passage, in complaints of the bladder occasioned by slimy matter, as well as in all nephritic pains. Questions whether it be gelatine.

"The efficacy of this remedy has long ago established its use;" and the writer adds, "that during my practice in pharmacy, I have prepared large quantities of it; so much were physicians and their patients satisfied with its operation." Its efficacy,

His method of preparing the gizzards was to choose those of young fowls, and particularly of pullets: after cleansing, rubbing, washing, and wiping them carefully, he strung them, and left them to dry on hurdles between sheets of paper, assisted by the gentle heat of a stove, and not in the sun, which, according to M. Pia, would have spoiled them. they should not be dried in the sun.

When the gizzards were properly dried, they became friable, almost transparent, and exhibited on being broken a vitreous appearance. Vitreous appearance.

The powder obtained was of a whitish grey ash-colour, yielding in the mouth a kind of mucilage, and possessing a slightly salt and bitter taste. Powder ash-coloured, mucilaginous and saltish.

This powder was administered twice a day, (morning and evening) in doses of twenty-four to thirty-six grains, in a glass of the infusion of pellitory of the wall; of bearberries (*uva ursi*) or of lintseed sweetened with syrup. Dose.

Effects. The efficacy of this remedy as a diuretic and aperient, was so much relied on, that the afflicted even omitted the infusion and took it in pure water with a little sugar.

Arguments in its favour. The long experience which has been had of the salutary effects of gizzard as a febrifuge, diuretic, aperient, &c. and the publicity which the government, doubtless not upon light grounds, has given to this remedy, are authorities in its favour; and it must therefore be an acceptable labour to the physician, to furnish him with new lights upon an object so essentially interesting to humanity. This is the motive by which I have been induced to submit the following experiments to the society of medicine.

Inquiries as to its composition and use. As gizzard has a great analogy to gelatine, I endeavoured to discover their similitude. If gelatine be really a febrifuge, gizzard should be so likewise, particularly as it contains, when fresh, a large portion of that substance; but whence does it derive its power as a diuretic, aperient, &c.? does it possess it in common with gelatine? I cannot tell. Or, have the saline parts of its composition this double property? of this also I am ignorant; for practice has not yet ascertained whether the anti-febrile quality should be ascribed to the acidulous salts rather than to the substances with which they are combined.

Experiments on recent gizzard. A fresh gizzard presented the following phenomena.

A. The water wherein this substance had been boiled acquired a yellowish white colour, and flakes were deposited in cooling; it had a taste rather insipid than sweet.

It reddened the tincture of turnsol.

B. Lime water, and water of barytes produced in this liquor an abundant precipitate, partially soluble by nitric and muriatic acids.

C. Ammonia caused a less degree of precipitation.

D. Oxigenated muriatic acid separated with flakes from the liquor.

E. Caustic potash, either solid or liquid, acted upon gizzard in the same manner as upon muscular flesh.

When ground together, ammonia was disengaged from the gizzard; it became soft, of a reddish colour, and soluble in water. If this liquor be evaporated, it will deposit fibres in cooling. Alcohol, by destroying the potash, separated a flaky substance, soluble in water.

This

This aqueous ſolution gave a precipitate on the addition of lime-water, or muriate of lime or of barytes, as well as of ſome acids. The precipitate obtained by lime-water may be rediſſolved by the addition of more water, which proves that the mixture had not become truly ſaponaceous, but that the potaſh had merely diſſolved the animal matter.

F. The action of certain metallic ſolutions on the liquor of freſh gizzard was more or leſs perceptible, according to the facility with which the metal communicated its oxigen to the animal matter.

Nitrates of mercury and of ſilver, for example, were decompoſed, but the precipitates obtained by the action of theſe ſalts upon gelatine and the extract, quickly turned black, particularly that of mercury, and they were no longer ſoluble in nitric acid. The oxides had, therefore communicated a part of their oxigen to the gelatine and the extractive matter, which were thus united to the mercury, now approaching a metalline ſtate.

Oxigenated muriate of mercury was not decompoſed in this manner. The circumſtances, in fact, were no longer alike: the exceſs of oxigen which it contains ſufficing to oxigenate the two ſubſtances. Here the precipitate was very little coloured, and the metallic ſalt was only reſtored to the ſtate of mild mercurial muriate.

Some other metallic ſolutions produced in the liquor of freſh gizzard only gelatinous flakes; ſuch are the acetate of lead, and the ſulphate of copper and iron.

G. Aqueous tincture of nutgall changed the liquor into a kind of jelly.

Experiments on dried gizzard.

I have thought theſe experiments ſufficient for demonſtrating the nature of thoſe ſubſtances which were capable of ſolution in water; yet as gizzard is not adminiſtered in its freſh ſtate, but undergoes a proceſs which might cauſe a variation in the foregoing reſults, I again examined it in this latter point of view.

In drying the gizzard, I followed the preſcription already cited of M. Pia, and obtained a ſubſtance exactly anſwering his deſcription.

A. Reduced to powder, its taſte was inſipid, yet partaking ſtrongly of an animal flavour; its colour was a whitiſh grey.

 B. The

Experiments on dried gizzard.

B. The aqueous decoction took a light yellow tint, and smelled like chicken broth.

It reddened the tincture of turnsol.

C. Lime-water and water of barytes caused the same kind of precipitate as in the decoction of fresh gizzard.

D. Oxalate of ammonia proved the presence of lime.

E. Oxigenated muriatic acid separated white flakes.

F. Nitric acid had a violent effect upon the dry gizzard; at a mild temperature it dissolved it completely.

Nitric acid at eighteen degrees excited a slight effervescence, and by gradually increasing its temperature, a separation was perceived of azotic gas, then of nitrous gas, and of carbonic acid gas.

The liquor left in the retort was evaporated, in the expectation of obtaining crystals; but on cooling, none appeared. The evaporation was then continued, the result of which was a yellowish glutinous matter, tenacious, and of an excessively bitter and acrid taste.

Water imbibed the acid, and presented all the characters of the decoction of apples.

G. Metallic solutions presented nothing particular, as in the experiments upon fresh gizzard, except that antimonial tartrite of potash was decomposed, forming in the decoction a white precipitate.

H. Aqueous infusion of nut-gall produced a less copious precipitate in this experiment, than it had with that upon fresh gizzard.

I. Dry and friable gizzard was digested in alcohol; but the liquor was scarcely coloured, even with the assistance of caloric.

This alcoholic tincture reddened that of turnsol, and gave precipitates with lime-water and water of barytes, as also with nitrate of silver; a proof that the alcohol has dissolved only the saline particles.

L. The incineration of gizzard left a residue of a saline and alcaline taste. Paper tinged by curcuma became of a deep brown.

This residue was partly soluble in water. The liquor contained sulphate, muriate, and carbonate of potash.

The part not soluble, on being submitted to the action of muriatic acid, discovered carbonate of lime, phosphate of lime, and a small portion of iron.

Hence

Hence it results, that the greater part of the salts contained in gizzard, is the acid phosphate of lime; the presence of muriate and sulphate of potash is also observable.

These salts are not only united with gelatine, but also with a small quantity of extractive matter. It should seem that the latter substance, and perhaps the gelatine, is oxigenated by the desiccation of the gizzard; for in this state they are less soluble in water.

Wishing to ascertain the difference between pure gelatine, and that which had been oxigenated, I made experiments upon the former, of which the following is the result.

Pure gelatine acquires different properties, according to the means employed in its oxigenation. Experiments on gelatin,

Of the metallic oxides, some freely communicate their oxigen to gelatine, as the oxide of red-lead, and the red oxide of mercury; but the gelatine was combined with a part of the oxide, and could not again be separated completely from it. In treating gelatine with the red oxide of mercury, a part of the oxide was restored to its metallic state, and the remainder assumed a reddish brown colour. with metallic oxides.

Superoxigenated muriate of potash heated with gelatine, caused no alteration in its nature. And other means of oxygenation.

Oxigen gas combined with it but slowly, and in small quantity. After being for a considerable time submitted to the action of this gas, the gelatine only suffered a change of colour; it became whitish, but its characteristics are still the same.

Oxigenated muriatic acid presented the following phenomena.

On pouring oxigenated muriatic acid gas into dissolved gelatine, a whitish thick scum appeared on the surface, of a moderate thickness, the under side of which gradually changed colour, and became milky. The white filaments which swam in the liquor, together with the scum which floated on the surface, were separated by filtering, and washed in cold and warm water till the water ceased to redden tincture of turnsol. The substance thus prepared presented the following characteristics:

1. It was capable of extension equally with gluten, and was of a white colour. Properties of oxigenated gelatin.

2. It was very light, and ſwam upon water.

3. When well waſhed, it retained little or no flavour,

4. Left expoſed to the air, it dried, and fell to duſt.

5. It did not redden the tincture of turnſol.

6. It was ſcarcely at all ſoluble in warm water. On boiling it a length of time, in a ſufficient quantity of water, it was reduced to an infinite number of particles, ſo minute as to be hardly perceptible; but as the heat was lowered, they re-united in a maſs as before the boiling.

7. Heated nitric and acetic acids diſſolved this ſubſtance; but it was precipitated in its original form, by refrigeration.

8. Trituration with cauſtic potaſh produced a ſeparation of ammoniac.

It differs from albumen.

This matter, it will be perceived, is neither gelatine nor albumen, ſince its properties are wholly different.

The gelatin of gizzard is probably oxigenated.

It appears probable, that the gelatine in gizzard acquires by drying, properties analogous to thoſe above deſcribed; which, with the changes obſerved in the extractive matter already mentioned, would certainly render dried gizzard leſs ſoluble in water.

We have no means of aſcertaining, for want of a proper object of compariſon, whether this difference be eſſential to the efficacy of gizzard; and I know not if freſh gizzard has ever been adopted in medical practice. I could only wiſh to aſcertain if its febrifuge quality exiſt in the oxigenated gelatine, in the extractive matter, or in the acid ſalt. Indeed, on comparing the quantity of gelatine adminiſtered to patients, according to M. Seguin, with the doſe of powdered gizzard, above-deſcribed, a great difference will be obſerved; and yet according to thoſe who have made uſe of it, a ſmall doſe of powdered gizzard is ſufficient to check the fever.

The compariſon which I have made of gelatine with gizzard is ſufficient to eſtabliſh a material diſtinction between them.

Experiments on gelatine.

Pure gelatine poſſeſſes a weak inſipid flavour; does not redden tincture of turnſol; is mucous and gluey between the fingers; aſſumes in the fire a concrete, ſolid, and tranſparemt appearance; and is ſoluble in boiling water.

Solution of barytes or of lime mixed with that of gelatine, cauſes a precipitation of phoſphate of lime.

Sulphates

Experiments on gelatine.

Sulphates of copper or tin, and acetate of lead, experience no decompofition.

Nitrates of mercury and filver are decompofed, but the precipitates are much lefs copious than thofe produced with the decoction of gizzards

Solution of tartrite of antimony only thickened the liquor.

Alcohol likewife has but little power over gelatine. The precipitates obtained by means of the water of lime or of barytes, as well as that by nitrate of filver, are fcarcely perceptible.

The decoction of frefh gizzard when fuitably evaporated, leaves a coloured gelatinous matter, foluble in water, which reddens tincture of turnfol; gives copious precipitates with lime-water and water of barytes; decompofes fulphates of iron and copper, acetate of lead, muriate of tin, tartrite of antimonial potafh, and nitrates of mercury and filver; the precipitates refulting from thefe decompofitions are generally too confiderable to be attributed folely to the gelatine.

Dried and powdered gizzard poffeffes characteriftics ftill more diftinct from thofe of pure gelatine, whence I conclude that the latter fubftance has a different operation.

I leave practitioners to decide on the advantages which the medical art may derive from gizzard; it is for them to decide whether much confidence is to be placed in the notice inferted in the Journal d'Economique. And if it fhall appear that the medieal ufe of this material has been attended with fuccefs; it will perhaps be proper to attend particularly to other fubftances which have not hitherto been fuppofed to poffefs any febrifuge virtue; fuch as the falts with excefs of acid, the oxigenated extractive and even oxigenated gelatine.

VI.

On Pirite found in France by M. Cocq, Commissary of Gunpowder and Saltpetre Works at Clermont-Ferrant, with an Analysis of this Substance. By J. J. DRAPPIER, *Teacher of Chemistry at the Polytechnic School.**

Crystals of pirite found in the district of Puy de Dome,

M. COCQ found the crystals of pirite, in a porous grey porphyry, with a base of feldspath, and containing crystals of quartz, forming a part of that chain of primitive mountains which support the volcanoes of the district of Puy-de-Dome. These crystals of pirite separate from the rock, and leave in the porphyry an impression perfectly smooth.

—and on the way to Menat.

He also found at the village of St. Avit, and in the vicinity of Pont-Gibaud, a substance which appeared to be pirite; in both situations it was so indeterminate as to render it impossible to pronounce exactly on its nature. But in returning to Menat, at twelve leagues to the north of Clermont, he perceived the granites resuming the same appearance of those which he had observed near Saint Avit and Pont-Gibaud, sometimes the colour of the feldspath inclined to purple, and oftentimes this substance appearing alone in the mass of the granite, exhibited a beautiful purple.

The grey porous granite appeared again at intervals, with the appearance of the crystals observed in the same rock near St. Avit and Pont-Gibaud; at last, after a great many searches, he found the pirite well defined, and assuming a character much more determinate than that of Scheenberg.

Its Physical Characteristics.

Each crystal is a prism of twelve faces, of a blackish or greenish brown, with a smooth surface.

Its colour is a greenish or blackish brown. Its form is a regular hexhedral prism, of which all the lateral edges are truncated, which constitutes it a prism of twelve faces. Sometimes the prism has also a small face at each of the angles of its base, which has not hitherto been remarked in the pirite of Saxony.

The surface of the crystals is smooth, and a little brilliant: in its interior, the pirite is dull, containing at times some particles of mica.

* Journal des Mines, Vol. XVII. p. 307.

Its

Its fracture is unequal, with a fine grain, approaching to a splintery fracture.

Its fracture is unequal, with a fine grain.

It admits of being scraped by a knife, and yields a dust of a bright grey colour; it is tender, and does not adhere to the tongue, though it is a little unctuous to the touch.

Yields to the knife, does not adhere to the tongue, and is a little unctuous to the touch.

Besides the size of its crystals, their faces, the substances to which they are found attached, added to the characters described, establish the identity of this mineral with the pirite of Saxony.

The crystals found in Auvergne are more perfect than those of Scheenberg; they exhibit no alteration, and the purity of their form removes all doubt of there being any necessity to class this substance as a new species.

The external characters of these crystals leave no doubt of their identity with the pirite of Saxony.

Analysis by M. Drappier.

The pirite of France, separated carefully from its bed, and reduced to a fine powder, is attacked and discoloured by muriatic acid. This acid dissolves the oxide of iron, the colouring principle, and a portion of the alumine: but as it leaves a considerable residue, on which it appears to have no action, M. Drappier thought the method of analysis should be changed: he then took 100 parts of this substance, and kept it at a red heat in a crucible of platina for half an hour; after it was cooled, there was a loss of seven parts. The remaining 93 parts were heated in the crucible for three quarters of an hour, with three times their weight of caustic potash, purified by alcohol. The fused mass, detached from the crucible by distilled water, dissolved entirely in muriatic acid. The solution evaporated almost to dryness, and then dilnted with a fresh quantity of water, let fall a white precipitate, having all the characters of silex. This precipitate washed carefully and well dried, formed 0,46 of the substance submitted to experiment.

Analysis of the French pirite.

One hundred parts lose 7 by heating.

The remainder fused with potash dissolves in muriatic acid.

The solution evaporated and treated with water deposits 46 parts precipitate.

The remainder of the muriatic solution was decomposed by caustic potash. It immediately formed a precipitate, which soon dissolved again in the excess of alkali, with the exception of 2½ parts of oxide of iron.

The residue treated with caustic potash leaves 2½ parts iron oxide.

The alkaline solution saturated by an acid, deposited 42 parts of an earth, which had all the properties of alumine. All these precipitates, before they were weighed, were washed carefully, and heated to redness in a crucible of platina.

The alkaline solution saturated by an acid deposits 42 parts alumine.

Analysis

Analysis of the Pirites of France compared with that of Saxony

Analysis tabulated.

	Pirite of France.	Pirite of Saxony analysed by Klaproth.
Silex - - - - -	46,00 - - -	29,50
Alumine - - -	42,00 - - -	63,75
Oxide of iron -	2,50 - - -	6,75
Loss by calcination	7,00	
Loss - - - -	2,50	
	100,00	100,00

Remarks on the difference of the pirite of France from that of Saxony.

M. Drappier thinks, on comparing his analysis with that made by M. Klaproth, that it may be concluded, supposing there was no error in either analysis, that either the pirite of France is not the same substance as that of Saxony, or that minerals having the same external characters, and especially the same form, may vary both in their chemical properties, and in the proportions of their constituent principles. M. Klaproth says that acids have no action on the pirite of Saxony, that he found much difficulty in operating on it by potash, and that, in order to separate its parts, he was obliged to treat it twice with this alkali. The same chemist appears not to have found any water in this substance. This difference, it is true, may be explained, if it is considered that the pirite of Saxony contains more alumine, and that it adheres to the tongue, while that of France has not this property, probably on account of the water which it contains.

VII.

Experiments, shewing, contrary to the Assertions of Morichini, that the Enamel of Teeth does not contain Fluoric Acid. In a Letter from WM. BRANDE, *Esq.*

To Mr. NICHOLSON.

SIR,

Gay Lussac on fluoric acid in animal substances.

HAVING seen in one of the last numbers of the *Annales de Chimie*, an article entitled, "Lettre de Monsieur Gay-Lussac a Monsieur Berthollet, sur le presence de l'acide fluorique dans les substances animales," &c. I was surprised to find that a chemist at Rome, of the name of Morichini, had discovered fluoric

fluoric acid united to lime in the enamel of human teeth. The extraordinary results of these researches, induced me to repeat them; but before I mention the experiments from which I have drawn conclusions different from those of the above-mentioned chemist, it may perhaps be proper to quote that part of Gay-Lussac's letter which relates to the present subject: —and said by Morichini to exist in the enamel of teeth.

"M. Morichini having detached some of the enamel from human teeth, supposed that it might bear some resemblance in its composition to the enamel of the fossil teeth of an elephant, in which, in a former occasion, he had detached fluoric acid; he therefore subjected it to analysis, and perceived, to his great satisfaction, that it contained a large proportion of fluoric acid. Quotation to that effect.

To render these experiments more conclusive, he submitted portions of the two species of enamel, viz. that of the fossil, and human teeth, and likewise fluat of lime, to the action of sulphuric acid, and found that the last of these three substances yielded fluoric acid in the greatest abundance, and that the enamel of fossil teeth yielded somewhat more than that of human teeth; but Morichini remarks, that this difference is merely owing to the presence of animal matter in the two kinds of enamel, and that the disengagement of the acid from the fluat may be retarded, by adding a little gelatine to that substance, after it has been calcined, and then drying the compound. He moreover observes that the vapours which sulphuric acid disengaged from any of these three substances, had the property of acting on glass, of depositing a siliceous film on water, and other properties, which it is scarcely necessary to mention. Morichini says that the enamel of recent teeth afforded fluoric acid as well as that of fossil teeth; and that the sulphuric acid disengaged vapours that corrode glass, &c.

According to Morichini's experiments, one hundred parts of the enamel of human teeth contain 30 parts of animal substance, and 22 parts of fluat and phosphate of lime, with some magnesia, alumine, and carbonic acid. He has not yet been able to separate the fluoric and phosporic acids from each other, but thinks that the proportion of the latter must be extremely minute. M. Morichini has also observed that the enamel of the fossil teeth of the elephant differs from that of human teeth, in containing a smaller proportion of animal substance and phosphoric acid; but he thinks that the phosphoric acid which he found in the enamel of human teeth may have been derived Component parts of enamel of teeth according to Morichini.

from

from a portion of the bony part from which the enamel is separated with great difficulty. But the most interesting and unexpected result is, that fluoric acid exists in animal substances: a discovery of the greatest importance. These experiments oppose the present opinion concerning the composition of enamel, for Mr. Hatchett in his analysis of this substance has only detected phosphate of lime.

Morichini professes to have proved my facts by repeated experiments.

The result of Mr. Hatchett's experiments, together with those which were subsequently published by Mr. Josse, in the Annales de Chimie, Tom XLIII. rendered it necessary for M. Morichini to submit his opinion to accurate investigation, and after having made a numerous series of experiments on the subject, he observes, that he cannot entertain a doubt, that the enamel of human teeth consists chiefly of fluat of lime.

General remarks by Gay-Lussac.

After some observations on the composition of ivory, M. Gay-Lussac concludes this part of his letter, by observing that there is an immense field laid open in that part of chemistry which relates to animal substances, if it were merely to search for fluoric acid. Morichini has undertaken an investigation of the subject: but so much remains to be done, that the exertions of many chemists will be requisite."

The author's experiments shew the contrary.

I shall now relate some experiments, which will shew that fluoric acid does not exist in the enamel of human teeth, but that this substance consists chiefly of phosphate of lime, as originally stated by Mr. Hatchett.*

Enamel of human teeth was ignited, pulverized, and subjected to sulphuric acid. The fumes did not corrode glass.

One hundred grains of the enamel of human teeth, detached from what is usually termed the bony part, but which appears to consist of a substance of the nature of ivory, were kept for a few minutes in a red heat, and then pulverised. The enamel, thus reduced to powder, was put into a platina crucible, in which a piece of a glass rod was placed horizontally in such a manner as to be about an inch and a half above the enamel. Half an ounce of sulphuric acid was then added, and the crucible being covered with a clean plate of glass, the heat of a lamp was applied, and distillation carried on for half an hour. During the process, white suffocating fumes were extricated; but on removing the glass which closed the top of the crucible, neither this, nor the rod below it were in the least acted upon; which certainly would have happened, had

* Vide Phil. Trans. 1799, p. 328.

any

any fluoric acid been present. Finding this, therefore, to be the case, I proceeded as follows:—Fifty grains of the same enamel were introduced into a small glass retort, and a little sulphuric acid being added, distillation was carried on nearly to dryness, but in such a manner, that the gaseous products might be received over mercury. A small quantity of sulphuric acid gas was disengaged, and what remained in the retort, consisted, as far as I could ascertain, of a mixture of sulphate of lime, phosphoric acid, and a small portion of sulphur, arising from a decomposition of a small part of the sulphuric acid by the animal matter, existing in the enamel.

neither was there any extrication of fluoric acid by adding sulph. acid to enamel and distilling over mercury.

I have the honour to be,

Sir,

Your most obedient servant,

WILLIAM BRANDE.

Arlington Street,
Feb. 15, 1806.

VIII.

A Memoir on taking the Levels of the whole Surface of France By P. S. GIRARD, *Chief Engineer of Bridges and Highways, &c.**

IF the surface of the earth were formed by the revolution of a curve round its axis, it would be sufficient, in order to determine the respective positions of different points upon it, to measure their distances from the intersection of that surface made by the plane of the equator and any particular or assumed meridian.

Thus geographers, considering the earth as perfectly spherical, have determined the position of any given place by the conjunction of two co-ordinates, one of which is the arc of the meridian, comprised between the place and the equator, and the other an arc of the circle parallel to the equator, comprised between the place and any assigned meridian.

The method by which geographers determine the position of a place.

* Journal des Mines, Vol. XVII. p. 297.

As

As these two co-ordinates intersect each other at right angles, it is apparent that the method of geographers, for determining the position of any place on the earth, is the same as that by which the position of a point on a plane is commonly determined.

is not exact, on account of the inequality of the earth's surface.

But this process, which would completely answer the views of geographers, if the terrestrial sphere were regular, ceases to be exact when the irregularities and protuberances are considered, with which the surface of this spheroid is covered.

This true position of a place is in a line perpendicular to that assigned by geographers.

The position of any place depends in reality, according to this hypothesis, on a third co-ordinate, which is supposed to be drawn perpendicular to the point of intersection of the two others.

This third co-ordinate ought to be taken vertically over the place of which the position is to be determined, and its measure reckoned from the place itself to its arrival at an imaginary surface, produced by the revolution of a known curve round the axis of the earth.

The level of the sea affords a spherical surface, from whence to measure those perpendiculars.

But it is known, that if our globe were surrounded by a fluid mass, all other force being supposed to be absent but that of terrestrial gravity, the surface of this fluid mass would be that of a spherical solid, of which the mean surface of the sea, in its actual state, represents a part: It appears then convenient to choose, for the third co-ordinate here mentioned, that portion of a vertical line passing through any place, which is comprised between that place and the mean surface of the sea, supposed to penetrate the globe and to be extended beneath the continent.

This is the best, though not the only method:

We have said that the choice of this line would be convenient; because, in reality, the position of a point on the terrestrial surface may be determined by adopting any other system of co-ordinates; for example, by fixing the position of this point, by three planes mutually intersecting at right angles;

But it is the most simple, and is besides generally adopted.

but, besides the advantage of greater simplicity in the expression of the circular co-ordinates, they have moreover, that of being generally adopted; for the geographical charts, hitherto prepared, may be considered as the projection of the continents and islands on the mean surface of the sea; so that there only remains, in order to render geography perfect, to add to the latitude and longitude of all the places on the earth, the vertical

The true position of a place determined by annexing its

vertical height which they are elevated above the surface of the ocean.

vertical elevation above the sea to its latitude and longitude.

The object of this memoir is to indicate the means of determining this vertical heighth, by their particular application to the territory of France.

Management proper for ascertaining these positions shewn in the instance of France.

It is evident, that all the operations necessary for this determination, may be reduced to a series of levels made in determinate directions.

Nature itself has pointed out these directions, by the lines of *greatest declivity*, which the large rivers, and those which flow into them, form on the surface of the earth.

Thus, France being divided into five principal basons, by the Rhine, the Seine, the Loire, the Gironde, and the Rhone, —the levels of the course of these rivers, from their sources, or from their entrance into France, to their terminations in the ocean, would form the first basis of the work proposed to be undertaken.

The levels of the five great rivers of France would form the basis of this operation for that country.

After having ascertained this first basis of the general operation, the levels of the streams by which the great rivers are supplied, should be next taken, and these streams should be considered without any regard to those of the third order, by which they are themselves maintained.

The levels of the streams which supply those great rivers should be next taken.

At the same time, the levels of the rivers of the second rank, which fall into the two seas, should be taken; such as the Escaut, the Somme, the Orne, the Vilaine, the Charente, the Adour, the Herault, &c.

The levels of the rivers of the second rank taken at the same time as those of the first.

The declivities of the beds of the secondary rivers being known, those of the rivers of the third, fourth, and fifth orders, &c. should be determined successively, according to special instructions which should be given for this purpose.

The levels of rivers of the third, fourth, and fifth order, taken.

By thus classing the operations relative to the general levels of France, and by arranging their results in order, as they were obtained, all the data would be soon collected, which were necessary for tracing the elevation of its territory on a geographical chart already prepared.

This tracing of the elevations would be effected, by joining all the points on one level by the same line.

Elevation of the surface of France to be expressed in the rough, by joining all the points on one level by the same line.

These *lines of levels* might be supposed to be elevated perpendicularly, one above the other, by a determinate space, conformable to the scale of the chart on which they were traced.

It is evident, that these lines would represent the borders of the coasts of the sea, if it was supposed that its mean level should be elevated successively to the same heighths which they represented.

M. Triel prepared the sketch of a map on this plan.

It was according to this idea that M. Dupain Triel prepared a physical chart, mentioned by M. Lacroix, member of the National Institute, in his introduction to Pinkerton's Geography; a chart which, from the defect of materials necessary for its construction, presented only the sketch of a work, the extent of which would require for its perfection an union of means, which could not be at the disposal of any particular individual.*

The order has been pointed out in which this work ought to be executed, and we shall now examine how it should be performed.

The courses of the great rivers should be divided into portions, and the levels of each taken by observers at the same time.

The bed of each of the great rivers must be divided into a certain number of portions, and each portion should be levelled by observers, who should operate at the same time.

These observers should place accounts of their operations at each extremity of the portions of the basons with which they were charged; and as the levels of the secondary rivers should be connected with those of the principal rivers, it would be necessary also to place accounts of the operations at the mouth of each of the influent streams.

The levels should be taken on the banks of the rivers, without any regard to the surface of the water. If it were thought useful to determine the declivity of this surface, it would be easy to ascertain it, by levels taken at the same time with the others, at certain distances from each other.

The results to be collected into a general system.

When the different observers have completed their respective observations, the results must be collected, to form the series of levels of one of the beds. And in the same manner the levels of all the rest should be obtained.

After this, a general system should be formed from those particular levels, by connecting together the different beds, by operations directed from one to the other, according to those lines which would afford the greatest facility.

* Compare this with Mr. Churchill's plan, at p. 224 of our XIth. Vol.—T.

There

There only remains to determine to what agents Government should entruſt the performance of the general levelling of France, in order to have it executed with the greateſt exactneſs, ſpeed, and economy.

The engineers of bridges and highways, already placed in the different departments, where this operation ſhould be performed, are evidently the only perſons to whom it could be confided, ſo as to fulfil theſe three conditions.

The engineers of bridges and highways would be the moſt proper perſons to employ in this work.

In fact, the execution of all projects relative to the eſtabliſhment of communications by land or by water, require, that the elevation in relief of the country, through which the works ſhould be carried, ſhould be known. The theory and practice of levelling form an eſſential part of the inſtruction given to the engineers of bridges and highways; and greater reliance may be placed on the exactneſs of the reſults which they might furniſh, becauſe the uſe of the inſtruments neceſſary to this operation, is more familiar to them.

On the other hand, there are none of thoſe engineers who could not dedicate ſome days of the ſummer to taking the levels of that portion of ſuch great rivers, or ſtreams, as ſhall traverſe his diſtrict; and as it is eaſy to take the levels of four or five kilometres (about three Engliſh miles) each day, eſpecially when the line to be levelled is previouſly determined by the direction of the river or current of water, it is certain, that the engineers of the bridges and highways might collect, in a very ſhort period, very minutely detailed materials for a phyſical chart of France.

Laſtly, theſe materials would be collected by them with the leaſt poſſible expence, becauſe Government would neither have to ſupport the coſt of extraordinary journies, nor the purchaſe of inſtruments, as the engineers are already, by the very nature of their employments, diſperſed over the ſeveral diſtricts where it would be neceſſary to operate, and are, at the ſame time, provided with the different inſtruments required for this purpoſe.

They could perform it without expence to the nation.

It may alſo be added, that the taking the general levels of France appears to be, with the more propriety, a work that ſhould be performed by the engineers of bridges and highways, as they would be the firſt to profit by this operation in putting their projects into execution.

It would tend much to their own benefit to have it effected.

Suppose then that the engineers of bridges and highways were charged with the performance of this work, let us consider how, after some years, the exactness of the results, which they had collected, could be sufficiently ascertained.

Let us take, for example, the bed of the Loire, whose course is of great extent.

The chief engineers of the departments of the Upper Loire, of the Loire, of the Saone and Loire, of the Nievre, of the Loiret, of the Loire and Cher, of the Indre and Loire, of the Mayenne and Loire, and of the Lower Loire, would be ordered to furnish, during the year, the levels of that part of the course of the Loire which traversed their respective departments.

According to the new organization of the service of bridges and highways, these nine departments require twenty-two engineers, in the district of each of whom would be found a portion of the work to be performed.

The total extent of the Loire is about ninety muriameters (about 550 miles), which being divided among twenty-two observers, would give to each of them little more than forty kilometers (about twenty-five miles) of levels to execute.

Levels of the Loire might be taken by the 22 engineers in that district, in one season.

There is reason to believe, from experience, that the twenty-two engineers employed on the course of the river, would finish, in less than one season, the levels of the whole river.

The same thing may be affirmed of the engineers placed in the departments traversed by the Rhine, the Seine, the Gironde, and the Rhone. It appears then beyond a doubt, that, at the end of the first year, the chief part of this physical chart could be completed, to which the farther details might be afterwards added.

Whatever care may be bestowed in taking levels, their verification is always an useful operation. That of the general levels of France might be made as often, and in whatever circumstances it should be judged necessary. It would be sufficient for this purpose, to direct the newly appointed engineers to repeat, in the departments to which they might be sent, the observations of their predecessors; which, besides the advantage of confirming or correcting the results already obtained, would give an opportunity to the new engineers of acquiring, in person, a knowledge of the elevation of their respective districts in relief.

Levels of France, when taken, might be afterwards verified by the newly appointed engineers.

The

The facility and promptitude with which the engineers of bridges and highways might execute this work, will be apparent, if it be recollected that, at the time when the major part of the great roads in France were formed, and when a general system of internal communications was desired to be established, M. de Trudaine, assisted by M. Perronnet, caused plans to be taken of all the principal roads, from their commencement to the frontiers. There was joined to the plan of the road properly so called, that of the country bordering on it, to the distance of three or four hundred yards at each side; a work which evidently required more time than simply taking the levels of a determined line, such as we propose; and yet the engineers of bridges and highways, or their pupils, employed in taking those itinerary plans, completed from five to six leagues of them each month. Facility of this work.

The general utility of the operation, of which a sketch is here given, will sooner or later determine some of the nations of Europe to undertake it. France, on whose territory has been lately executed some of the finest geological operations which were ever performed, and where, for the first time, a system of universal measure has been established on an invariable basis, seems to be particularly called on, to give, on this occasion, the first example of a work, which, by completing the natural geography of countries, will furnish new facts to geology, and to those different parts of natural history which depend on it. Geological operations already performed in France, urged as a motive to commence this work; which would complete the natural geography of countries.

IX.

Observations and Experiments on the Composition of Water, and other Elementary Doctrines. By H. B. K.

To Mr. NICHOLSON.

SIR,

As two papers have appeared in your Journal, both of which militate against the result of my experiment, and as Mr. Accum has been concerned in one of them, I therefore think it incumbent on me to answer them.

The gases obtained from water by galvanism, smelled of nitrous gas after explosion, and gave nitre with potash.

I was glad to see my experiments in your Journal, as it has so extensive a circulation. I shall now give you the analytical part to confirm my former experiments; as by them I had, I hope, given strong evidence, that acids are necessary in forming gases. Having collected a great quantity of the gases produced by the galvanic pile, I introduced them into a strong glass tube, closed at one end, the other end I afterwards closed, having previously introduced to the gases a small quantity of a solution of potash: through this tube the electric spark was made to pass, it having small openings to admit the wires of communication. Upon their combustion, the smell of the nitrous acid vapour appeared, both from its colour and smell; and the tube being moved up and down, so as to allow the vapour and the solution of potash to come in contact; the solution being examined some short time after, it gave evident and unequivocal signs of the nitrate of potash.

On Pacchioni and Riffant's experiments.

I see in your Journal, Mr. Riffant's experiments, in answer to Mr. Pacchiani's paper. Indeed, in reading Mr. P.'s experiments, nothing could appear more vague and wild than that water, by having oxigen, the supposed acidifying principle, taken from it, should become a strong mineral acid. Mr. Riffant's second experiment directly contradicts my experiment, on the supposition that water is a compound body; but if examined upon my supposition, that the acids are necessary in forming the gases, and that the water is only necessary in forming the water of composition, I hope I shall be able to prove that his experiment confirms my opinion. There were very little of gases formed by this experiment, and the wires were very much calcined: now this calcination was from the acid, or acids, I proved by repeating the very same experiments; but only instead of distilled water, I used a solution of potash, and instead of the wires being calcined, they were not sensibly acted upon, and the potash became nitrated.—Now, Mr. Nicholson, I (seriously and ardently) call upon your numerous readers to perform this experiment, which I think must be decisive.

Against the doctrine of the composition of water: it is urged that the gases obtained in galvanism vary from different causes.

I can but smile at the French chemists, in making the proportion of the gases so exactly to tally with their opinion of the composition of water; but I have in my experiments found very different results; the kind of gases depending a good deal upon the wires used, the different metals, their length, and the

the different liquors between the plates of the pile; all of which had a sensible effect upon the gases, both upon the quantity produced, and their kind; the calcinable wires when along producing the most inflammable kinds, and the less calcinable metals the more of the oxigen kind, and the longer the wires the more in volume were the gases.

Remarks on Mr. Northmore's experiments.

Mr. Northmore, in your Journal, endeavours to prove the formation of the nitric acid from the compression of gases.—Upon investigation, his experiments will, I think, be found very vague and inconclusive: that gases from active compression will produce both heat and water, has been long known. The first experiment was in condensing hidrogen, oxigen and nitrogen gases, two pints of each. He says they produced "white floating vapours, probably the gaseous oxide;" but in experiment the seventh, he observes, "the hydrogen produced white clouds at first, *quære* ammonia." So without any chemical examination of these white clouds, they are at first supposed to be the gaseous oxide, and afterwards ammonia, just according as his theory dictates to him. In the fifth experiment, he says, "and the result was only a smell of gaseous oxide of nitrogen, a few yellowish fumes." Here then the gaseous oxide produces a yellowish colour, though in the first experiment it was a white colour.

The acid produced was, from the same vague opinion, supposed to be the nitric; but this he endeavours to examine in the next experiment; first by a good test, in exposing it to lime water; and he says, "Some yellow particles were seen floating upon the lime water; these particles probably arose from the resinous substance used in fastening on the cap of the receiver being dissolved by the nitrous gas formed during condensation. Here then was the lime water affected. I say with confidence, these flocculi in the lime water were from the carbonic acid produced, and why they appeared yellow was from their being seen through the gases, being clouded with an orange colour, which, as he observes, they put on when they were condensed.

That acids are necessary in forming oxigen gas, I hope appears very clear from my experiments; therefore when it forms combustion with inflammable bodies, it is rational to suppose that an acid will appear upon its decomposition. If the combustion is active, as in the French experiments in condensing oxigen

Remarks on Mr. Northmore's experiments.

oxigen and hidrogen gases, the heat produced is so active as to make an explosion of the gases; but if a slow combustion, it will leave the oxigenized acid in a gaseous state, as carbonic acid gas, which, I suppose, was the case in Mr. Northmore's experiments. His next experiment of examining the acid: He compressed the gases upon two scruples of the solution of potash; he says, "there was scarce enough acidity to tinge the edge of the test paper; of course, I could not effect the formation of the nitrate of potash." But always to assign some reason for the failure, he says, "This quantity (of gases) was hardly sufficient for the receiver's capacity;" but there was the same quantity in this experiment as in the others; nay, in the next experiment (the sixth) there was identically the *same quantity and in the same proportions*; and in this fifth experiment, he found so little acid, as he says, "Scarce enough acidity to tinge the edge of the test paper; of course I could not effect the formation of the nitrate of potash." Now upon the supposition that the carbonic acid was formed, it would unite with the potash, and therefore the mixture would be less saturated with it: But if the acid was so strong as he speaks of in the sixth experiment, from the very same process, as he says, "Which moisture was strongly acid to the taste, coloured litmus, and when very much diluted with water, acted upon silver." Now if Mr. Northmore will consult the writings of chemists, (Dr. Black's lectures, for instance); in Vol. II. the doctor says, "that the nitric acid requires a little water to reduce it to the strength of aqua fortis; in order to act upon silver, therefore, in this experiment, the acid must have been in the concentrated state of the nitric acid, as it required water to be diluted to make it act upon silver; but probably Mr. N. does not know that water impregnated with hidrogen gas will colour silver; which I suppose to have been the case here.

This reasoning must appear to be most extraordinary: this vast quantity of nitric acid produced was even to penetrate into the cap of the receiver; but very unfortunately for this supposition, chemists are of opinion that acids will not dissolve resins. Mr. Hatchett has promoted their operation upon each other by using the strong nitric acid; but this was a difficult and tedious process, not during the transitory action of a little time, by compression; and where the resin was so concealed;

cealed; so that the acid could not get to act upon it, being placed within the cap of the receiver; therefore the small quantity of weak acid formed in Mr. N.'s experiment could not rationally be supposed to have penetrated to it, even if it was in a high concentrated state; but it must have been much diluted with water, as there was water also produced in this experiment: Also, if it was in this high concentrated state, and in that abundance as to enter into all the crevices, it would easily have been detected, and his fifth experiment was for this purpose, but it failed; he could find no nitrous acid.

Remarks on Mr. Northmore's experiments.

In experiment the seventh, he supposed he had formed ammonia, and he says in this very experiment, "Some vapour was generated, which was, as usual, strongly acid." How comes it that this acid, which was supposed to find out the resin, so perfectly concealed, could not find out the ammonia, which was formed along with it in the process, and so universally dispersed as to form white clouds.

The third experiment: "Two pints of carbonic acid and two of hydrogen was subjected to condensation. The result was a watery vapour, and a gas of rather offensive smell." This compressed gas I found to be similar to Mr. Cruikshanks's gaseous oxide of carbon from the acid air and the phlogistic air saturating each other.

Mr. Northmore apologises for giving these experiments "until he had brought them to a greater degree of perfection," but at the conclusion he also says, "Besides the above, I have made various other experiments with different gases, &c." But as he says nothing more of these *imperfect experiments*, there are no hopes of his correcting them; he appears to have exhausted his research, and we have seen with what success. There appears such an ardent desire to support the Lavoisierian theory: but if it has always failed from the experiments of Lavoisier himself, Mr. Cavendish and others, I am afraid we have little to expect from these new supporters. I might make many other observations, but these will, I presume, be thought enough.

It will be expected in contradicting Mr. N.'s experiments I should make some of my own; I must own my apparatus was not so good as his; yet I hope sufficient to prove my opinions. I had the barrel of a large blunderbuss, and stopped its priming hole, and having filled it either with sand or distilled

Experiments of compression of the gases.

diſtilled water, I then tied to its mouth a bladder filled with the different gaſes I wanted to compreſs. Upon pouring out the ſand or water into the bladder, the gaſes entered the barrel, and then having a ſtrong iron ram-rod made perfectly air-tight, it was forced down upon the gaſes by a long iron lever, by which means I was capable of making a ſtronger condenſation than reducing them to one fifth of the volume.

They gave out heat and moiſture.

The reſults of theſe experiments were, that all the different gaſes, by being compreſſed, gave out heat and moiſture: The hydrogen gas, the greateſt proportion of moiſture to its ſpecific gravity.

Oxigen and hydrogen gave acid.

That when oxigen and hydrogen gaſes were compreſſed, there was an acid which produced flocculi in lime water; and that nitrogen gas was not neceſſary to the production of the acid, but rather retarded its production. The nitrogen gas obtained by the nitric acid and animal ſubſtances ought not to be uſed in theſe experiments, as it is partly acid of itſelf; but the nitrogen of the atmoſphere ought to be made uſe of, being previouſly paſſed through lime water.

Mr. Nicholſon, I have condenſed this communication as much as poſſible, in order that it might not occupy too much room in your Journal. I am

Your's, &c.

H. B. K.

London, February, 15, 1806.

X.

On the Conſtruction of the Sails of Ships and Veſſels. By MALCOLM COWAN, *Eſq. Captain in the Royal Navy.**

The ſails of ſhips have been long without improvement.

IT appears from the conſtruction of the ſails of ſhips and veſſels, belonging to every nation, that it is a ſubject no one has hitherto taken much pains to inveſtigate; but the maritime world have been content to uſe them, as they found them, every one following the beaten track of his predeceſſors, without examination.

* Extracted from an eſſay, by the author, who has letters patent for the ſails.

That

That the fails of fhips have been hitherto fo conftructed by all European nations, fo as to be only managed with great labour and danger; and that when managed with the greateft fkill, they are very far from being of that utility which they ought to poffefs, and are capable of having, is inconteftible.

They are worked with difficulty and danger.

Ships are driven on fhore every winter, which might, with proper fails, have efcaped every danger. The lofs of one fail, in many fituations, is followed by the inevitable lofs of the fhip and crew. Sails are often fplit in hauling up to reef, and it may be neceffary to reef a fail that is worn, to preferve it from fplitting; hence the neceffity of the fails being conftructed to reef without ftarting tack or fheet.

Many fhips have been loft by not having time, or drift, to haul their courfes up, to reef them on the yard, by which they rifk their fplitting; a circumftance which alone muft convince the feamen of the utility of having fails that can be reefed without taking their effect off the fhip.

Many dangers may be avoided, by carrying fail with fafety to the mafts and yards. A fhip can carry top gallant fails that reef at the foot, with fafety, when other fhips muft furl theirs; an evident advantage in many fituations.

Advantages of fails which are reefed at the foot inftead of the head.

The top fails of fhips, with one or two reefs at the foot, can be reefed in a minute by one feaman at each lower yard arm, while they remain fet with the top gallant fails over them, by only fettling the hallyards; by which a fhip in fqually weather, on many occafions, would have a great advantage, particularly in chace, &c. or when caught by a fudden fhift of wind on a lee fhore, or obliged to haul fuddenly to the wind from failing large.

The facility with which fails that reef at the foot, can at all times be managed, would enable fhips to make quicker voyages, and prevent them often, when weakly manned, from detaining fleets; by the difficulty and danger of carrying fail, being entirely removed, muft enable merchant fhips to be navigated with fewer hands, which would be a confiderable faving of expence, and a great advantage in time of war in particular, when men are fo fcarce.

The feams ought to be horizontal.

If the fails were made with horizontal cloths and feams, the fails would ftand better, particularly in a gale of wind; as the ftrongeft direction of the cloths and feams would be oppofed to the greateft force of the wind, which acts horizontally;

tally; and ſhould the ſail ſplit in that direction, it would ſtill remain full, and be leſs liable to blow away altogether, which is generally the caſe when a ſail ſplits in a vertical direction. Storm ſtay ſails ſet purpoſely with the cloths horizontal, have proved this beyond a doubt.

Many ſeamen are loſt every winter, by falling overboard from the yards while reefing the ſails, as it is more dangerous and requires longer time to perform in a gale of wind, than furling the ſails, which is not ſo often neceſſary as reefing.

Other advantages from improvements in ſails, &c.

Ships may ſometimes avoid a lee ſhore, by carrying a timely preſs of ſail, and when in that perilous ſituation, in a gale of wind, the ſafety of the ſhip may ſolely depend on the ſails being kept ſet; though it may be neceſſary to reduce them, either to ſave them, or eaſe the ſhip. The common ſails require to be hauled up, to be reefed, at the riſk of ſplitting them, at a time perhaps, when the ſhip is in imminent danger, from the want of ſea room; and the beſt ſeamen of the crew muſt be ſent on the yards when they poſſibly may be much wanted on deck.

Whole fleets are often caught by a ſudden ſhift of wind, of a lee ſhore, thrown into confuſion, and obliged immediately to reef their ſails, at the ſame time the ſhips may require the whole of their crews on deck, to attend the working of the ſhip, to keep clear of each other; particularly when it happens in the night time, with the wind ſqually and variable.

When ſhips from foreign voyages, enter the Engliſh or Iriſh channels, in the winter time, when the days are ſhort, and the nights long, with weak or diſabled crews, or men not accuſtomed to cold or froſt, ſuch as Laſcars, Negroes, &c. it is with the greateſt difficulty they can be prevailed on to go aloft; but ſhould they get on a lee ſhore, which all ſhips are liable to, and with a helpleſs crew, nothing can exceed the horror of their ſituation, ſhould they not be able to proportion their ſail to the wind in time to ſave the ſhip.

Naval improvements are of great importance to the ſtate.

To facilitate the working of ſhips, by the moſt approved means, is an object of greater conſequence to a maritime nation than many are aware of, even in a commercial point of view. The little alteration that has been made in ſhipping for many years paſt, ſhews with what indifference attempts at improvements have been regarded; many of which have been

been tried, proved, and neglected, while others have failed from the unavoidable expence, neceſſarily attending all experiments on a large ſcale, which require repetitions to bring to perfection; or from partial intereſts or prejudices, being oppoſed with ſucceſs (which not unfrequently happens) to improvements of general advantage. And many are apt to ſuppoſe that particular arts and ſciences are brought to the higheſt degree of perfection they are capable of, though experience every day convinces us to the contrary.

The largeſt ſhips might be much more eaſily navigated, if the improvements on capſterns, windlaſſes, blocks, hawſe-holes, &c. were univerſally adopted from the great reduction of the friction.

The following explanation will be eaſily underſtood by thoſe who are acquainted with the conſtruction of a ſhip, See Plate VI.

The courſes and top gallant ſails are to be reefed from the deck, and the top ſails by one man at each lower yard arm.

Deſcription of the improved ſails.

A. The after-part of the ſails.

B. A ſtrong band on the after-part of the ſails, ſewed on at the upper part only, and roped at the lower part.

C. The long clews of the courſe, formed by the bight of the leech rope and rope of the reef band with thimbles, ſeized in above the tack blocks, for laſhing the lower clews to.

D. The tacks and ſheets fitted to the upper clews of the courſes with thimbles above the tack blocks.

E. The buntlines, brought up through the thimbles H, on the foot ropes of the ſails, and bent to the cringles I, on the ropes of the reef bands.

F. A ſmall rope or gaſket, rove, occaſionally as a reef line, through eyelet holes, under the reef bands, and made faſt to the middle ſail, for confining the ſail when reefed, in the wake of the reef bands.

G. Thimbles in the clews and earings.

K. Thimbles on the foot rope with the earings rove through them.

L. The reef tackle pendants, paſſing through thimbles in the clews and leech of the top-ſail, and brought up and bent to the cringles above the upper reef band.

M. A boom tackle or burton hooked to the reef pendants.

N. The crow-foot legs to the top gallant buntline.

N. B. The reef bands are fewed by the upper part, to the after part of the fails, to prevent the rope from girting the fails, when the whole fail is fet.

The rope of the reef band of the courfe, is the fame fize as the common foot rope, and the foot rope muft be in proportion to the rate of the fhip: for the firft rates, $3\frac{1}{4}$, or 4 inch; fecond rates $3\frac{1}{2}$; third rates, 3 inch rope: as the quantity of fail below the reef band does not require fo ftrong a foot rope, as when the whole fail depended on it.

The rope of the reef bands of the top fails, fhould be fmaller than the leech ropes, as the foot of the fail will be confiderably ftrengthened, when reefed.

Thefe fails are not fo heavy as the common ones; a 74 gun fhip's courfe is reduced in weight about 200lbs. as the points, bands, and eyelet holes of the old reefs are not required, nor any additional geer.

Men of war will find one reef at the foot of the top fails, very ufeful in chace in fqually weather, or when obliged to haul fuddenly on a wind, &c.

Merchant fhips will only require two reefs in the top fails, as the fquareft part of the fail is taken off, by reefing at the foot inftead of the head, but more reefs may be added if neceffary.

Inftructions for reefing and fetting the fails.

When the courfes are to be reefed, caft off the lower clews, from the thimbles in the upper clews, haul up the flack fail by the buntlines. and haul tort the reef line, one part at a time, from the middle of the fail, towards the clews, and make it faft round the upper clews, fo as to confine the lower clews.

To fet the fail, reeve a few turns of the lafhing for the clews, and haul them down, overhauling the reef line, and buntlines.

To reef the top fails, fend a man up to each lower yard arm, fettle the hallyards, and haul the fail down by the reef tackles, and pafs the turns of the earings, through the thimbles in the earing cringles, and on the foot of the rope, and make them faft. Hoift the fail tort up, haul through the flack of the buntlines, and haul tort the reef lines on each fide towards the clews, and make faft,

The top gallant fails are reefed from the deck, by the clew lines, and a fingle buntline with a crow-foot.

The

The buntlines and reef line will confine the flack fail, when reefed, clofe up the wake of the reef bands; and the buntlines will only require to be kept hand tort, as is ufual, to prevent them from chafing the fail.

The flack fail of the roof of the top fail, will be kept extended tort acrofs the foot, by the reef pendants paffing through cringles in the leech.

The ends of the clewlines may likewife pafs through cringles, in the leech of the top gallant fails if neceffary.

The reef lines, if neceffary, may be in feparate pieces, made faft in the middle and quarters of the fail.

XI.

Experiments on condenfed Gafes. *By* T. NORTHMORE.

To Mr. NICHOLSON,

SIR,

I NOW take the liberty of prefenting you with a continuation of my experiments upon the condenfation of the gafes, but firft beg leave to make one obfervation, viz. that the quantity of gas faid to be injected in each experiment, cannot (particularly in the preceding article) always be depended upon; for its tendency to efcape is fo conftant and powerful, as frequently to elude every effort of mine to prevent it, and if it can find no other exit, it will fometimes efcape by the fide of the pifton of the forcing pump. In the preceding experiments I have endeavoured as much as poffible to obviate this evil, but not always with the fuccefs that I could wifh. Experiments on condenfed gafes.

Repeating the eighth experiment mentioned in my former letter, (fee Vol. XII. p. 372-3) viz. the condenfation of nitrogen upon lime*, in order to difcover the caufe of the lofs of colour in the nitrogen, I perceived that this arofe from its fixation, and a nitrate of lime was the refult. This experiment, on account of the elafticity of nitrogen previous to its change of habitude, requires fome caution; for one of my beft receivers, three-eighths of an inch thick, was Nitrogen condenfed upon lime, produced nitrate.

* Your marginal note fays erroneoufly *lime-water*.

fhivered

shivered in pieces with a violent explosion, after I had set it aside to see the effect of time upon the compressed gas.

Nitrogen and gaseous oxide of carbon condensed, gave nitrous acid, &c.

Experiment 9. Upwards of a pint of nitrogen was condensed, and upon this I pumped one pint of gaseous oxide of carbon. The colour of the nitrogen was destroyed; nitrous acid was formed; and upon collecting the liberated gaseous oxide, it burnt not unlike alcohol. The two gases together were at first highly elastic.

Explosions attributed to nitrogen.

From the facility with which nitrogen becomes united and fixed in various bodies, and from its expansive force when liberated from that state, I know not whether I am sufficiently warranted in suggesting an opinion, that the explosive force of various compounds may in a great measure be attributed to the sudden liberation of this fixed gas. To this cause I partly attribute the fulminating silver of Berthollet; the fulminating gold, and various nitrates; and the detonation which accompanies the decomposition of ammoniac by oxigenated muriatic acid gas.

Attempt to fire phosphorus by condensed air.

Exp. 10. Having been unsuccessful in my endeavours to inflame phosphorus by the compression of atmospheric air, (see *Exp.* 4.) I now tried oxigen, but with little better effect. The phosphorus appeared to be somewhat discoloured, and I thought had a tendency to liquify, as it does when put upon a heated plate of iron. Indeed I have no doubt that some heat is generated by the condensation of air, since the thermometer rises upon external application to the receiver.

Oxigenated muriatic acid gas gave a yellow and highly volatile fluid by condensation.

Exp. 11. Upon the compression of nearly two pints of oxigenated muriatic acid gas in a receiver two and a quarter cubic inches capacity, it speedily became converted into a yellow *fluid*, of such extreme volatility under the common pressure of the atmosphere, that it instantly evaporates upon opening the screw of the receiver. I need not add, that this fluid, so highly concentrated, is of a most insupportable pungency. When atmospheric air was pumped into the empty receiver, it was speedily filled with dense white fumes. There was a trifling residue of a yellowish substance left after the evaporation, which probably arose from a small portion of the oil and grease used in the machine, mixed with some of the concentrated gas; it yielded to sulphuric ether, and destroyed vegetable colours.

This

This gas is very injurious to the machine, and on that account difficult to work.

Oxigenated muriatic acid and oxigen afforded a thicker fluid.

Exp. 12. Upon half a pint of oxigen was injected one pint of oxigenated muriatic acid gas. The result was a thicker substance which did not so soon evaporate, and a yellowish mass was left behind.

Oxigenated muriatic gas and nitrogen.

Exp. 13. Upon half a pint of nitrogen was injected one pint of oxy-muriatic gas. The result was a still thicker substance, and the yellow colour deeper, nor did it appear to act so powerfully upon vegetable colours. Much of the grease of the machine was carried down in both these last experiments, which formed part of the yellow residue, and yielded only to ether.

Receiver burst, caution.

Exp. 14. Having condensed about a pint of carbonic acid, the receiver very unexpectedly burst with violence. This circumstance I attribute to the vicinity of the furnace, and I mention it to guard others against standing too near a fire in these experiments; nor perhaps may it be useless to add another precaution, that of using goggles, or at least a thick plate of glass when examining the results.

Carbonic acid and oxigenated muriatic acid.

I now took a new receiver of three cubic inches of capacity, and pumped in one pint of carbonic acid, and upon this rather more than a pint of oxigenated muriatic acid gas.

The union produced a light sap-green colour, but no fluid, though as usual the oil of the machine had retained enough efficacy to destroy vegetable colours.

Oxigenated muriatic acid gas on hidrogen.

Exp. 15. Upon rather more than a pint of hidrogen, which was highly elastic, were compressed two pints of the oxigenated muriatic gas. The result was a light yellow-green colour, and no fluid. Some smoke or vapour seemed to issue out of the receiver upon turning the screw, and the gas was highly destructive of colouring matter.

Muriatic acid gas easily made fluid by condensation.

Exp. 16. I now proceeded to the muriatic acid gas, and upon the condensation of a small quantity of it, a beautiful green coloured substance adhered to the side of the receiver, which had all the qualities of muriatic acid; but upon a large quantity, four pints, being condensed, the result was a yellowish-green glutinous substance, which does not evaporate, but is instantly absorbed by a few drops of water; it is of a highly pungent quality, being the essence of muriatic acid. As this gas easily becomes fluid, there is little or no

elasticity

elasticity, so that any quantity may be condensed without danger. My method of collecting this, and other gases which are absorbable by water, is by means of an exhausted florence flask (and in some cases an empty bladder) connected by a stop-cock with the extremity of the retort.

An idea here occurs to me, that the facility of fixation which is the property of the compressed muriatic, oxy-muriatic, and some other gases, may be made of some utility to the arts, since by previously pouring in a little water, or other fluid into the receiver, an acid may be obtained of almost any degree of concentration.

Sulphureous acid gas condensed by pressure.

Exp. 17. Having collected about a pint and a half of sulphureous acid gas, I proceeded to condense it in the three cubic inch receiver, but after a very few pumps the forcing piston became immoveable, being completely choked by the operation of the gas. A sufficient quantity however had been compressed to form vapour, and a thick slimy fluid of a dark yellow colour began to trickle down the sides of the receiver, which immediately evaporated with the most suffocating odour upon the removal of the pressure. This experiment corroborates the affirmation of Monge and Clouet, mentioned in Accum's chemistry, vol. I. p. 319. viz, that " by extreme artificial cold, and a strong pressure exerted at the same time, they rendered sulphureous acid gas fluid. From the injury which this gas does to the machine, it will be very difficult to perform any experiments upon its elective attractions with the other gases.

I remain, Sir,

Your obedient humble Servant,

T. NORTHMORE.

Devonshire Street, Portland Place,
Feb. 15, 1806.

On

XII.

On the Probability that Muriatic Acid is composed of Oxigen and Hidrogen. In a Letter from Mr. J. Martin.

To Mr. NICHOLSON,

SIR,

LATE experiments in galvanism have furnished sufficient grounds to suspect, that the muriatic acid is an oxide of hidrogen, and I have been somewhat strengthened in this supposition by the well known fact, that hidrogen gas is always liberated upon effecting a solution of tin in muriatic acid: this phenomenon has been accounted for, by supposing the water which held the muriatic acid in solution to be decomposed; its oxigen seizing the metal which thereby became disposed to be taken up by the acid and the hidrogen, the other constituent part of the water being liberated under the form of gas: however plausible this hypothesis might seem, I did not think it perfectly satisfactory, for it the acid consisted of oxigen and hidrogen, part of the oxigen might unite to the metal to render it fit to be dissolved by the remaining acid, and its hidrogen of course given out under the gaseous form, in this case no decomposition of the water would take place, or at least these phenomena might happen without that decomposition. To clear up these doubts I procured an earthern tube into which was introduced some iron wire; the tube was made to traverse a furnace; to the one end was luted a bent tube, brought under the shelf of a pneumatic trough, and to the other was adapted a tubulated retort, containing some muriate of soda carefully freed from its water of crystallization When I supposed the iron wire was sufficiently ignited, I affused some dense sulphuric acid over the muriate of soda; as soon as the atmospheric air which the vessel contained was nearly expelled, hidrogen gas was liberated from the other extremity of the tube in considerable quantities, mixed however with a small portion of muriatic acid gas; after the operation had been suffered to go on some time, the apparatus was taken to pieces, and crystals of muriate of iron were found in the tube. May we

Facts induced in favour of the position, that muriatic acid may be an oxide of hidrogen.

Experiment. Muriatic acid gas disengaged from decripetated sea salt by sulphuric acid, was passed over ignited iron. Hidrogen was liberated.

It is inferred that this came from the acid.

we not from this experiment be fufficiently authorized to conclude, that muriatic acid is compofed of oxigen and hidrogen, and that hidrogen gas is liberated in confequence of part of the oxigen of the acid uniting to the metal to predifpofe it to unite to the remaining acid?

It is to be remarked, that the hidrogen gas was liberated in fuch abundance as to do away every idea, that it might proceed from any water which the gas accidently held in folution.

Should you deem thefe obfervations of fufficient value, an infertion of them in your valuable journal will greatly oblige,

Sir,

Your moft obedient,

and moft humble Servant,

J. MARTIN.

Crown-Court, Old Broad Street,
February 20, 1806.

XIII.

Subftance of a Memoir read before the Society of Emulation, at Amiens, by Meffrs. REYNARD *and* FACQUER, *on the foul Air of Oil Cifterns* *.

Fatal effects of the foul air of an oil ciftern.

ACHILLE POULAIN, foap-maker at Amiens, and one of his workmen having been killed by the foul air of an oil ciftern, into which the latter had fallen in an attempt to cleanfe it, and the former in endeavouring to fave the man's life, Meffrs. Reynard and Facquer were induced to make an analyfis of the deleterious vapour which had caufed this melancholy accident.

Dimenfions of the ciftern.

The ciftern meafured about twelve feet in every direction. Its mouth is fecured with a fmall cover which completely excludes the external air.

Appearance of the oil.

The vegetable oil, of which only a fmall quantity at a time had been depofited in this ciftern, was thick, vifcid, and even in fome places gelatinous, yielding a ftrong rancid effluvium.

* Annales de Chimie, Vol. LVI.

A lighted

A lighted candle on being let down into the ciftern, was inftantly extinguifhed.

The furface of lime-water, when included for a few minutes in a broad veffel, was flightly tinged with prifmatic colours.

To obtain the gas for experiments, bottles filled with water were lowered into the cifterns, and emptied at various depths.

On the gas obtained from about two feet below the mouth of the ciftern, the following experiments were tried: Obfervations on the gas.

1. A cylindrical veffel being filled with the gas, kept in contact with lime-water, during fifteen days, with frequent fhaking, caufed a fmall diminution in the bulk of the gas.

2. The fame experiment repeated with ammoniac offered a fimilar refult.

Thefe two experiments denote the prefence of carbonic acid gas.

3. The gas remaining from the two former experiments, when put in contact with liquid hydrogenated fulphuret of potafh, underwent an abforption of eight centimes; which muft have been oxigen.

The gas taken within a foot of the bottom of the ciftern afforded fimilar refults, only the proportion of carbonic acid gas was greater. That which remained after the effect of reagents was azote, as the following phenomena prove. It contained carbonic acid.

1. A lighted candle was extinguifhed by immerfion in the gas at the upper part of the cylindric veffel; but it remained burning if the veffel was previoufly opened for a few feconds.

2. The veffel when reverfed loft none of the gas contained in it; and the light was extinguifhed when introduced.

3. The luminous combuftion of phofphorus in oxigen gas (the formation of nitric acid with this gas and oxigen gas not having been tried) was confidered a pofitive proof of its nature.

This noxious gas was found to contain, Analyfis.

Upper Part,		Lower Part,	
Azotic gas - -	86	Azotic gas - -	80
Oxigenated gas - -	8	Oxigenated gas - -	6
Carbonic acid gas -	6	Carbonic acid gas -	14
	100		100

 The

Chemical agency infufficient to deftroy the foul air.

The nature of this gas does not admit of purification by lime or ammonia. Thefe indeed deftroy the carbonic acid, but have no influence on the azote.

Mechanical means more effectual.

Mechanical means are the only methods by which any confiderable quantity of this air can be fpeedily removed; fuch as the firing of gun-powder, the ufe of ventilators, &c.

Deftructive effects of confined air caufed by the prefence of azotic gas.

The refult of this analyfis is rather furprifing, as, inftead of a fuperabundance of carbonic acid gas, which was fuppofed to be the caufe of the deftructive effects of this confined air, azotic gas has been found—a gas lighter than atmofpheric air.

Theory.

The theory of this refult feems to be, that the oil having deprived the enclofed air of its oxigen, leaves only the azotic gas at liberty.

XIV.

Extract from a Memoir, by Meffrs. FOURCROY *and* VAUQUELIN, *on the Phenomena obferved in, and the Refults obtained from Animal Matter, when acted upon by Nitric Acid. Read at the National Inftitute, by* A. LAUGIER.*

Berthollet's experiments on azote,

THE exiftence of azote in animal fubftances has been determined by the experiments of M. Berthollet, and the difengagement of this principle, when treated with nitric acid, is among the most ufeful of modern difcoveries in chemiftry.

—repeated.

Meffrs. Fourcroy and Vauquelin, on repeating thefe experiments on mufcular fibre, have added fome interefting refults to this valuable fact.

The following is a fummary of their experiments, and of the refults which they obtained.

Nitrous acid with mufcular flefh gave azote and fome carbonic acid.

SECT. 1. A mixture of 150 grammes of mufcular flefh, with an equal quantity of nitric acid, at 32 degrees, and water, put into a mattrafs, and heated till it boiled gently, gave 96 cubic inches of gas, containing nine-tenths of azote, and one-tenth of carbonic acid.

The refiduum contained fibrous matter, yellow liquor, and a greafy fubftance.

The refiduum confifted of, 1, Matter which had not loft its original fibrous formation; 2, a yellowifh liquor; 3, a greafy fubftance, of a yellow colour, which floated on the furface of the liquor.

* *Annales de Chimie*, Vol. LVI. p. 37.

After

After separating the greafe, and filtring the liquor, the refidue was fubmitted to the following experiments.

To boiling water it gave a yellow colour, and the property of reddening vegetable blues: After wafhing in feveral waters, it continued to turn the colour, though it ceafed to give acidity. Wafhing rendered its colour deeper than at firft; and when diffufed in a little water, it ftill reddened paper of turnfol.

Its folution in alcalis was of a deep blood colour. It was precipitated by acids in yellow flakes.

This matter feels fat and pitchy; has a rancid fmell, and very bitter tafte. The fibrous matter refembles fat.

The fufion and fwelling which it undergoes when placed on hot coals, the greafy vapour, and fetid colour, produced by this operation; the fmall quantity of coal which it leaves, fhews its refemblance to fat fubftances, notwithftanding its acidity.

Sect. 2. On a clofer inveftigation of the yellow matter, the following characteriftics and properties were obferved:

It fo faturated alcalis as nearly to mafk their properties.— Its combinations with potafh and ammonia lathered like foap and water, and are not decompofed by carbonic acid, but precipitated the folutions of mercury and lead in yellowifh white flakes. It faturates alkalis.

The yellow matter decompofed alcaline carbonates, in the cold, with effervefcence, and likewife the acetate of potafh, with the affiftance of water, and a gentle heat. Decompofes carbonate.

The authors of the memoir next made ufe of alcohol, and found that the yellow matter was compofed of a fmall quantity of fat, which was taken up by the alcohol; and of an acid, which, on account of its colour, they denominated "*yellow acid.*" This acid, when deprived of its fat, which occafions an alteration in its properties, was of a deeper colour, more readily reddened the paper of turnfol, did not melt in the fame manner as before, nor exhale the fame rancid fmell, but fetid and ammoniacal vapours. It is a yellow acid and fatty matter.

The yellow acid is diffolved in the fat, to which it communicated acidity and rancidnefs. It combined with ammonia, and deprived it of its fmell; and by diftillation it yielded all the products of animal fubftances. Its conftituent principles, therefore, are azote, hydrogen, carbon, and oxigen; and it muft be placed among animal acids. The yellow liquor is an animal acid, confifting of azote, hydrogen, carbon and oxygen.

Sect. 3.

Sect. 3. The combination of yellow acid and fat, on being again submitted to the action of nitric acid, at a temperature of about 50 degrees, underwent no remarkable alteration.—Its colour changed from yellow to white; its specific gravity was diminished, as was likewise its bulk; but without any motion or effervescence in the acid. Blue colours were deeply reddened by it; it dissolved, as before, in the ley of potash, to which it communicated an orange-red colour, and had an extremely acrid taste. The action of nitric acid upon this yellow matter seems confined to giving it properties which make it approximate to an oily state, without destroying its original acid character.

Experiments on the nitric acid wherein the muscular flesh had been decomposed.

Sect. 4. It was of importance that the nitric acid with which the muscular flesh had been decomposed, should be examined. Its yellow colour resembled that of the solution of chromate of potash. When saturated with carbonate of potash, the liquor at first acquired an orange colour, afterwards it became turbid, and deposited a small quantity of orange-red powder. On distillation, this mixture afforded a clear liquid, void of colour, of a rancid smell, containing a little ammonia, probably formed by the nitric acid. What remained in the retort, was of a blackish brown colour, but it was not farther examined.

A colourless liquor, having the same taste and smell, was afterwards obtained by distillation of another portion of the nitric acid used in the decomposition of the muscular flesh. The liquor remaining in the retort became yellow by concentration, and its re-action upon nitric acid was quickly perceived in a copious emission of red vapours. When reduced to 40 grammes, flattish crystals were formed in a thick mother-water, whose tenacity was similar to that of the solution of gum.

This mother-water possessed an acid bitter taste, and on the addition of a little caustic potash, became of a blood-red colour: mixed with alcohol, it deposited a white flaky sediment, which afterwards formed itself into fine semi-transparent grains, of a pleasant acid flavour.

Five decigrammes of this salt, on being calcined, left 21 centigrammes of yellowish very light residuum, which effervesced and were dissolved in nitric acid, and on being evaporated produced crystals of sulphate of lime and nitrate of potash.

This

This faline precipitate, obtained by means of alcohol, was afcertained to be a mixture of fulphate of lime and acidulous oxalate of potafh.

The mother-water, after precipitation with alcohol, gave a fecond precipitate with lime-water, confifting of oxalate of lime. After this double operation with alcohol and lime-water, the mother-water, on being gradually evaporated, became converted into the brown vifcid fyrup, of a bitter tafte, like that of walnut fhells. This being mixed with a good quantity of alcohol, coagulated, and threw down a plentiful precipitate of white matter. This matter was very pure malate of lime, the alcohol having retained the yellow acrid fubftance.

Conclufions.

The learned authors of the memoir, of which we have given this detailed extract, conclude from the facts above ftated,

1. That the mufcles contain potafh, lime, and fulphuric acid, or perhaps fulphur burned by nitric acid.

2. That a portion of the mufcular fibre, or rather the cellular membrane with which it is enveloped, was converted by the action of the nitric acid into oxalic acid and malic acid.

The alcohol employed in the feparation of the malate of lime, held in folution, 1, A fmall portion of nitrate of lime; 2, A very bitter red-brown matter, poffeffing the flavour of walnut rhinds, of which more will be faid hereafter; 3. A fmall quantity of that detonating matter already found in indigo: it was in this cafe obtained by concentrating the alcoholic folution, and feparating it by the addition of carbonate of potafh, in the form of granulated cryftals, very inflammable, and very detonating.

Importance of the foregoing analyfis.

SECT. 5. The importance of the refults obtained from the foregoing analyfis will be readily underftood; particularly if a comparifon be made of the knowledge hitherto poffeffed, with the extenfive notions here opened to the view, of an object fo interefting in the confequences which may be drawn from it, in the applications which may be made to the animal economy, and which, as will be fhewn, leaves fcarcely any thing more to be defired.

Difcoveries added to what was formerly known on this fubject.

The difengagement of azotic gas, the formation of carbonic acid, of fat, of oxalic acid, and of a bitter fubftance, conftitute the whole that was known refpecting the treatment of

animal ſubſtances by nitric acid; to this is now added the diſcovery, 1, Of a yellow inſipid matter, of little ſolubility, though acid, and which immediately ſucceeds the fleſhy fibre; 2, Of another yellow matter, bitter, more ſoluble, and equally acid, which remains diſſolved in the nitric liquor; 3, Of an inflammable, detonating ſubſtance, which is alſo retained in ſolution; 4, and laſtly, of the formation of malic acid.

It appears, and is the opinion of Meſſrs. Fourcroy and Vauquelin, that the yellow and nearly inſoluble matter is the firſt degree of change produced upon the muſcular fibre; it paſſes quickly to the ſecond degree of alteration and of acidity, whoſe product is the more ſoluble yellow matter: this, by a third degree of alteration is ſucceeded by the inflammable detonating ſubſtance, being the third and laſt term of the decompoſing action of nitric acid. The authors of this memoir attribute the ſucceſſive formation of theſe three compounds to the ſubtraction of part of the azote, and of a more conſiderable portion of the hydrogen: by this means the proportions of their elements are changed, and there remains an exceſs of carbon and of oxigen, which produces the ſtate of fat and acidity already noticed. As to the proportion of the conſtituent principles of theſe three compounds, it is a problem of too remote a nature for its ſolution to be readily diſcovered.

Acidity of the yellow ſubſtance not cauſed by nitric acid.

Meſſrs. F. and V. examined if the acidity of the yellow ſubſtances might in any meaſure ariſe from nitric acid; but, after a careful inveſtigation, they were ſatisfied that it was in no degree preſent.

Formation of oxalic and malic acids.

The formation of oxalic and malic acids belongs to the white mucous ſcales of the cellular membrane. Comparative experiments of the effects of nitric acid on the white membranaceous organs, which furniſhed plenty of theſe acids, and very little of the fat yellow matter, led the authors to this concluſion.

SECT. 6. A few inſulated facts, which hitherto have ſcarcely appeared to be ſuſceptible of any uſeful application, ſeem to unite with thoſe preſented by this analyſis; and the learned chemiſts, to whom we are indebted for it, have not omitted to connect them with the other facts. Such are thoſe which are obtained by examining the bilious concretions in certain animals; thoſe in the gall-bladder of the ox, and elephant; and the analogy which appears to exiſt between bile,

thr

the colour of the ſkin in perſons afflicted with the jaundice, and alſo their urine, and the yellow ſubſtance treated of in this memoir. Analogy of the yellow matter to bile, jaundice, &c.

New experiments made with a view to confirm theſe ſuſpicions obtained the moſt happy reſults. The red matter of bilious concretions, when ſeparated from the bitter green matter with which it is combined, diſplayed ſimilar properties with the firſt yellow matter obtained from muſcles acted upon by nitric acid. Bilious concretions.

From the urine of a young man troubled with a ſlight jaundice, they obtained a red ſubſtance, whoſe identity with the matter formed by muſcles and nitric acid was remarkable. To obtain this, they evaporated the urine to the conſiſtency of honey, and treated the reſiduum with alcohol: this contained, beſides much of uree, ſal-ammoniac, and acetate of ſoda, of which the patient made uſe, the red ſubſtance they ſought for. It was found in the urine of an icteric ſubject.

From theſe experiments, made with ſkill and ability, may we not conclude with the authors, that the jaundice is occaſioned by a ſuperabundance of this matter introduced to the cutaneous abſorbent ſyſtem; that this is what gives a yellow colour to bile and bilious calculi, which diſplay, on analyſis, the ſame properties; and that the yellow acid is diſperſed throughout the animal economy, either by the oxigenation of the muſcular fibre, or of the ſanguineous fibrine, from which it is formed? Jaundice occaſioned by a ſuperabundance of the yellow acid; which alſo cauſes the yellow colour of bile, &c.

Neither can we avoid admitting a ſtriking analogy between this yellow acid matter, and the acid found in fat after long expoſure to the air, or that has contracted a yellow hue through diſeaſe, and fat treated with nitric acid to form oxigenated pomatum. Reſemblance of the yellow acid and rancid matter of fat.

It muſt be confeſſed that theſe conjectures aſſume much probability, when we conſider that the acetate of ſoda, alcaline carbonates, and yolks of eggs, are the remedies beſt adapted for the cure of the jaundice, and form alſo the beſt chemical ſolvents of the yellow acid, or of the acid and fat matter, which ſo evidently characteriſe the jaundice. Other facts.

After what has been ſaid, it muſt no longer be imagined that the hope of tracing the cauſe of morbific affections, is altogether chimerical: nor that diſcoveries in chemiſtry, and attentive Chemical reſearches not to be neglected by phyſicians.

reſearches

refearches refpecting animal matter, will not enlighten the phyfician on the nature of difeafes, and the means of curing them.

XV.

Remarks relative to Dr. HERSCHEL's *Figure of Saturn. By* AN OBSERVER.

To Mr. NICHOLSON.

SIR,

Singular circumftance that Dr. Herfchel's figure of Saturn had not been before obferved.

ON reading in your Journal, Obfervations on the fingular Figure of the Planet Saturn, by Dr. Herfchel, from the Philofophical Tranfactions; when I faw the engraving of the figure, as defcribed by the Doctor, refembling a parallelogram, one fide whereof is the equatorial and the other the polar diameter, with the four corners rounded off, fo as to leave both the equatorial and polar regions flatter than they would be in a regular fpheroidical figure; I was furprifed to find, on enquiry, that fo remarkable a figure had not been noticed before by other aftronomers, whofe telefcopes were fuppofed to define objects very correctly, with powers confiderably exceeding 160 times, by which power the Doctor could diftinguifh Saturn from the fpheroidical figure of Jupiter.

Former obf. of the Doctor did not fhew it.

In the year 1776, the Doctor relates he perceived the body of Saturn was not exactly round, and in 1781, that it was flattened at the poles, at leaft as much as Jupiter. In 1789, the Doctor being then prepoffeffed with its being fpheroidical, he meafured the equatorial and polar diameters, and fuppofed there could be no other particularity to remark in the figure of the planet.

It is evident, from the Doctor's former obfervations of Saturn and Jupiter, that the vifible difference in their figures was not, before laft year, obferved fo diftinctly, owing to the fuperior excellence of his 10-feet telefcope of two feet aperture, but that, when obferved, he afterwards found the other telefcopes gave a fimilar difparity.

Q. whether there was no deception in the telefcopes.

As the figures given by former aftronomers, and even by the Doctor himfelf, of both Jupiter and Saturn, were fpheroi-

dal,

dal, it may be requisite, before any intricate researches are attempted (as mentioned by the Doctor at the end of the communication), to be well assured that his telescopes have defined the figures of the planets accurately, which at present admits of a doubt, and which may be cleared up about the time of the next opposition of the Sun and Saturn, in April next.

The following may prove the necessity of such an enquiry:

Place a circular or spherical figure before a concave mirror, which mirror must be so inclined, that when the object is above the head of the observer, it may be seen, by reflection, in the center of the mirror *: If seen within the focus, the object will be represented oval in a vertical direction, and when beyond the focus, in a horizontal; which figure will be more and more oval as the angle is enlarged.

Experiment. An object seen by oblique reflection from a spherical mirror, is rendered oblong.

Your's,

AN OBSERVER.

XVI.

Experiments on a Mineral Substance formerly supposed to be Zeolite; with some Remarks on two Species of Uran-glimmer. By the Rev. WILLIAM GREGOR.†

THIS mineral is raised in a mine called Stenna Gwyn, in the parish of St. Stephen's, in Branwell, in the county of Cornwall; the principal production of which is the compound sulphuret of tin, copper, and iron.

Description and analysis of a mineral from Cornwall.

Description.

Two species of this mineral are found, assuming a marked difference in external character.

The first and most common one consists of an assemblage of minute crystals, which are attached to quartz crystals, in tufts, which diverge from the point of adherence, as from a centre. These tufts vary, as to the number of crystals, of which they

* If the object is small, it may be enlarged by a concave eye-glass.

† Phil. Transf. 1805.

Description and analysis of a mineral from Cornwall.

are composed, and are light and delicate in the forms which they assume, or they are grouped together according to a variety of degrees of proximity and compactness. Sometimes they fill the whole cavity of a stone, with little or no interruption; in other specimens they are seen partially spreading over the sides and pointed pyramids of quartz crystals.

In some cases these grouped tufts adhere very pertinaceously to the stone which bears them; in others, they are easily separable, in comparatively large pieces, from the quartz, the impressed form of which the pieces thus separated retain. The surface of these, which was in immediate contact with the quartz, exhibits the several minute crystals of which the mass consists, matted together in various directions.

These crystalline assemblages are, in general, white; a nearer inspection of the individual crystals proves that they are transparent. Sometimes they are stained of a yellowish hue by ochry water.

The size of these crystals varies considerably in different specimens. Sometimes they assume the appearance of a white powder raised up in small heaps, upon the surface of the stone, to which they adhere. In other specimens they resemble a tender down. And the larger sort varies, in relative size, in the proportion, perhaps, in which a human hair, horse-hair, and a hog's bristle, severally differ from each other in magnitude. They seldom exceed a quarter of an inch in length. The figure of these crystals is not easily ascertainable, on account of their minuteness. By the help of a very powerful microscope, they appear to consist of four-sided prisms; where these are broken off, the section exhibits a rhomboidal, approaching indeed to an elliptical figure, from the circumstance of the angles of the prism being worn away; but that the prism itself is rhomboidal, cannot be inferred from hence, unless we could be certified, that the section were at right angles with the axis of it.

Imbedded amongst these crystals two species of crystalline laminæ are frequently discoverable; the one consisting of parallelopipedon plates with truncated angles, applied to each other, of a green colour of various tints, from the emerald to the apple-green: the other species, consisting of an assemblage of square plates, which vary in thickness. The angles of the several square laminæ, which are applied to each other, are

not

not always coincident. They are of a bright wax yellow. The sides of the largest of these square laminæ is about a quarter of an inch. This last species is frequently found adhering to the sides of quartz crystals, in the cavities of granite.

Description and analysis of a mineral from Cornwall.

The other species of this mineral consists of an assemblage of crystals closely compacted together in the form of mammillary protuberances, in general, of the size of small peas, intimately connected with each other. A stratum of these about $\frac{1}{8}$ of an inch thick, is spread upon a layer of quartz, in the cavities or fissures of a species of compact granite. The striæ of which these mamillæ consist, diverge from a centre, like zeolite. Some of the individual striæ, in some cases, overtop their fellows, in these globular assemblages, and evidently assume, on their projecting points, a crystallized form.

A.

(1.) The detached crystals of the former species are easily reduced to powder, of a brilliant whiteness. At the temperature 56° of Fahrenheit, its specific gravity was found to be 2,22.

(2.) The hardness of the more compact species is sufficient to scratch calcareous spar. At the temperature 55°, its specific gravity was 2,253. It does not imbibe water.

(3.) Some of the crystals exposed, on charcoal, to the flame of the blowpipe suddenly and strongly driven upon them, decrepitate: if they are gradually exposed to the flame they grow opaque, and become more light and tender: but they show no signs of fusion under the strongest heat.

(4.) The phosphate of soda and ammonia takes up a piece of this mineral without effervescence, but it swims about the fused globule, unaltered. Borax dissolves a fragment of a crystal, and the globule remains transparent.

(5.) Some of this mineral, reduced to a fine powder, was mixed with about half its weight of pounded quartz, and kneaded with water into a ball; but as soon as the mass became dry, all cohesion was destroyed, and it fell into powder.

(6.) Sulphuric acid, poured upon some of it, caused no effervescence, nor was there any perceptible vapour extricated.

(7.) Some of the pulverized crystals were put into a crucible of platina, and sulphuric acid was poured upon them. The

Description and analysis of a mineral from Cornwall.

crucible was covered with a piece of glafs, and placed in warm fand. On examination of the crucible and its contents, after fome time, it appeared that the greater part of the mineral had been diffolved, but the furface of the glafs cover was not in the leaft affected.

(8.) Some of the cryftals were introduced into a fmall glafs retort, to which a receiver was adapted. The retort was expofed to the heat of a charcoal fire. A fluid diftilled over into the receiver, which had a peculiar empyreumatic fmell. It changed litmus-paper to a faint red. It produced no change in a folution of nitrate of filver; but it caufed a white precipitate in a folution of nitrate of mercury. I attributed thefe phænomena, at the time, to a fmall bit of the feather with which I had fwept the powder into the retort, and which, I thought, had fallen into it. A flight whitifh cruft was alfo produced in the neck of the retort, but the fmallnefs of the quantity did not admit of examination.

(9.) Some of this mineral, expofed to a red heat for about ten minutes, loft in weight at the rate of 25⅛ *per cent.* Another portion, expofed to a ftronger heat for more than an hour, loft 30¾ *per cent.* This operation was performed in a crucible of platina; the cover of which gave fome indications as if a flight portion of the finer parts had been volatilized.

Some of the compact fpecies, after expofure to a red heat for one hour, experienced a diminution in weight of 30 *per cent.*

(10.) The fulphuric, muriatic, and nitric acids, aided by a long digefting heat, effect nearly a complete folution of this fubftance. The quantity of the undiffolved refiduum is diminifhed in proportion to the purity of the mineral employed.

(11.) The nitrate of filver, as well as the muriate of barytes, produce no change in the folution of this fubftance in nitric acid.

(12.) The folutions of this fubftance in muriatic and nitric acids, cannot be brought to cryftallize.

B.

(1.) I felected fome of the cryftals of this fubftance, as free as it was poffible from extraneous matter. 50 grains grofsly pounded were expofed, in a platina crucible, to a red heat for

one hour. They weighed, *whilst still warm*, $35\frac{7}{8}$ grains, which is a loss of $28\frac{1}{4}$ *per cent.* 25 grains of the same parcel, from which I had taken the former, exposed to a heat of longer continuance and greater intensity, were diminished in weight, at the rate of $30\frac{3}{4}$ *per cent.* Description and analysis of a mineral from Cornwall.

(2.) The powder still preserved its pure whiteness. It was transferred into a matrass, and nitric acid poured upon it, which soon began to act upon it. The matrass was placed, for many hours, in a digesting heat. A solution of the whole of the substance, except a small portion, was effected; I added a few drops of muriatic acid, and continued the digestion.

(3.) The acid was now diluted with distilled water, and poured off from the residuum, which consisted partly of a fine spongy earth, and partly of fragments of quartz. It was caught on a filter and sufficiently edulcorated. The last portion of edulcorating water dropped through the filter of an opalish hue.

The residuum, dried and exposed to a red heat, for ten minutes, = $\frac{3}{16}$ of a grain, $\frac{1}{16}$ of which consisted of fragments of quartz, $\frac{1}{32}$ was found to be silica, and $\frac{3}{32}$ alumina.

C.

(1.) The clear solution and edulcorating water were poured into a large matrass and boiled, and whilst boiling, the contents were precipitated, in white flakes, by ammonia

(2.) When the ammonia had ceased to produce any further precipitate, the clear fluid was decanted, and assayed with carbonate of ammonia. But its transparency was not in the least disturbed.

(3.) This clear fluid, together with the edulcorating water, with which the subsided precipitate had been washed, was gradually evaporated. When its volume was considerably diminished, a separation of a spongy earth took place, more copiously than I had reason to expect, and the quantity of it was still further increased by a few drops of ammonia. This earth, thus separated, was sufficiently edulcorated, and added to the former precipitate.

(4.) The fluid was again evaporated, and at last transferred to a crucible of platina, and the salt reduced to a dry state: on redissolving this salt in distilled water, a minute portion of

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earthy matter was ſeparated, which, after edulcoration, was added to the reſt. The fluid from which it had been ſeparated, and the edulcorating water, were again evaporated to dryneſs, and the ammoniacal ſalt expelled by heat, in a platina crucible.

(5.) After the crucible had been made red hot, it was examined. I diſcovered on the bottom of it, ſome traces of earthy matter, and ſome ſpots, which had a glaſſy appearance. Water boiled upon it, diſſolved nothing; from which circumſtance, the abſence of both of the fixed alkaline ſalts may be inferred. Neither did nitric acid produce any alteration. A few drops of ſulphuric acid effected a ſolution of the ſubſtance, which adhered to the bottom of the crucible. Ammonia precipitated from it a ſmall quantity of earth, which was transferred to the reſt, and the ſulphate of ammonia and edulcorating water were again evaporated and expelled by heat. A few ſpots of the ſame glazing ſtill appeared. I had obſerved the ſame phenomenon in a former experiment: but in that, as well as in the preſent inſtance, the ſubſtance was in too ſmall a quantity to become the ſubject of experiment.

D.

(1.) Upon the precipitate (C 1), and the earths collected at different times, whilſt they were in a moiſt ſtate, I poured a ſolution of potaſh in alcohol mixed with diſtilled water; in a ſhort time, the greater part of it was diſſolved.

The clear ſolution was decanted, and the undiſſolved ſediment was transferred to a baſon of pure ſilver, and boiled with a ſolution of potaſh.

(2.) When the potaſh ceaſed to act upon it, it was diluted with diſtilled water and decanted from a brown powder, which had ſubſided. This powder edulcorated, dried, and ignited weighed $\frac{7}{16}$ of a grain; $\frac{1}{4}$ of a grain was alumina, $\frac{3}{32}$ ſilica, and $\frac{3}{32}$ oxide of iron.

E.

(1.) The ſolution effected by potaſh was decompoſed and rediſſolved by muriatic acid, and the contents of the ſolution were precipitated by ammonia. The ſubſided precipitate was edulcorated.

(2.)

(2.) The fluid and the edulcorating water were evaporated to drynefs, and rediffolved in diftilled water. Here again, to my furprife, a feparation took place of a white earth, more abundant than is ufual in cafes where ammonia is employed as a precipitant.

Defcription and analyfis of a mineral from Cornwall.

(3.) This earth and the precipitate were edulcorated with diftilled water, until it ceafed to affect a folution of nitrite of mercury. Collected, dried, and ignited, for one hour it weighed *whilft ftill warm* 32 $\frac{8}{16}$.

F.

(1.) This earth was placed in a crucible of platina, and repeatedly moiftened with fulphuric acid, which was abftracted from it in the fand bath; diftilled water affected the folution of the whole, except a white powder which weighed, after ignition, 2$\frac{1}{32}$ grains. It was proved to be filica.

(2.) This folution was now mixed with fome acetat of potafh and gradually evaporated; large and regular cryftals of alum were from time to time formed. A fmall portion of filica which weighed after ignition $\frac{1}{32}$ of grain was depofited; fome fulphate of lime alfo made its appearance, which wafhed with diluted alcohol and dried in a low heat $\frac{7}{16}$ of a grain.

(3.) A portion of the fluid remained which neither the addition of potafh nor the lapfe of many weeks could induce to cryftallize. Sufpecting that it might contain glucine, I precipitated the contents by carbonate of ammonia, added to excefs, and fhook the mixture repeatedly and ftrongly. The precipitated earth was collected and the fluid boiled, but it was found to contain nothing but a minute portion of alumina.

(4.) The edulcorated earth was rediffolved in fulphuric acid, except $\frac{5}{8}$ of a grain of ignited filica.

The folution was mixed with a little potafh, and gradually evaporated. Sulphate of lime was feparated at feveral times and after long intervals, which fufficiently wafhed and dried in a low heat $= \frac{9}{32}$. Some filica alfo feparated, but too minute in quantity to be afcertained by weight. The remaining fluid at length cryftallized into regularly formed alum.

(5.) The whole, therefore of the 32 $\frac{8}{16}$ (E 3.) confifted of alumina except 2$\frac{7}{8}$ of filica, and the lime contained in $\frac{23}{32}$ of fulphate of lime, which may be eftimated about $\frac{3}{16}$ of a grain; the alumina, therefore, $=$ 29; the alumina in B. and D. $= \frac{11}{32}$

 the

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the silica in B, D, and F, $= 3\frac{1}{16}$; the oxide of iron (D.) $= \frac{4}{16}$, and lime F, $\frac{3}{16}$; the volatile parts of this substance $= 15\frac{3}{8}$ in the 50 grains employed.

The sum total of these is - - -	$47\frac{1}{16}$
Loss - - - - - - - -	$2\frac{15}{16}$
	50

I have subjected these crystals, as well as the harder species of this mineral, to analysis by means of direct solution in sulphuric acid, and have found in each case the same fixed ingredients, viz. alumina, a small portion of silica, and a very minute quantity of lime. Both these latter ingredients are, I think, essential to the composition of this fossil, as I have always discovered them in the purest specimens. In this mode of analysis I experienced the same difficulty and tediousness of delay in bringing the last portions of the solution to crystallize into alum. This anomalous circumstance I have reason to attribute to a particular combination, which takes place between the sulphate of alumina and lime, silica, and potash. In my examination of the compact species there was no appearance of the sulphate of lime until the last; and in every experiment, previously to the fresh appearance of crystals of alum that had been long delayed, silica and sulphate of lime were deposited.

I forbear entering into any further details concerning my former experiments on this curious fossil, as I have reason to think that it will still require a more particular and minute examination, on account of another ingredient which eluded my notice, and which may possibly impart to its peculiar character. The scarcity of it has been hitherto a great bar to my experiments; I shall record, however, a few facts which I have lately observed, in the hope that at a future time I may be able to resume my examination of it.

I was induced to pay more attention to the volatile ingredients of this substance *. With this view, I introduced some

* Mr. Humphry Davy, whose well known skill and sagacity have probably rendered the researches of another person superfluous, had, I found, been engaged in the analysis of a mineral which is thought to be identical with the subject of these observations. He informed me that he had observed a peculiar smell, and acid properties in the water distilled from the substance which he examined.

of the crystals into a small retort, adapted a receiver unto it, and exposed the retort to a charcoal fire. The neck of the retort was soon covered with moisture, which passed into the receiver; and I observed a white crust gradually forming in the arch and neck of the retort.

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On examination of the fluid in the receiver, it was found to have the same empyreumatic smell that I had observed before. It resembles very much the smell which that fluid is found to have which is distilled from the white crust that surrounds flint as a nucleus.

It changed litmus paper to a faint reddish hue. It produced no change on a solution of nitrate of silver, and scarcely a perceptible one, on that of nitrate of mercury.

The crust formed in the neck of the retort consisted of thin scales, which after the vessel had been dried, were disposed to separate from the glass in some places, but in others they firmly adhered unto it. They were opaque, like white enamel, and reflected the colours of the rainbow. A portion of this substance exposed to the flame of the blow-pipe upon charcoal turned at first black, and then melted into a globule, that exhibited somewhat of a metallic splendor which soon grew dull. This substance is soluble in water; on evaporation of it, it assumes, at the edges of the fluid, a saline appearance, which, as the moisture evaporates, becomes earthy, opake, and white. Some of the solution changed litmus paper to a faint red. Lime and strontian waters produce in it white clouds, which a drop of nitric acid removes. Muriats of lime and barytes produce no change in it. Nitrate and acetate of barytes disturb its transparency, the effect produced by the latter is more evident. Nitrate of silver produces no effect, but nitrates of mercury and lead cause copious precipitates, which are white and soluble in nitric acid. Phosphate of ammonia and soda produced a white precipitate. Oxalate, tartrite, and prussiate of potash did not affect it, nor did sulphate of soda. Ammonia was dropped into it, but the fluid preserved its transparency. But carbonate of ammonia instantly caused a white precipitate, which was not redissolved by an excess of the precipitant; upon some of this subsided precipitate a concentrated solution of potash was poured and shaken with it, but it was not

 sensibly

Description and analysis of a mineral from Cornwall.

fenfibly diminifhed. But if after edulcoration it be diffolved in nitric acid, and potafh be added, no precipitate is produced.

Carbonate of potafh caufes a white precipitate when dropped into the aqueous folution of the fcaly fublimate.

The fupernatant fluid was poured off and gradually evaporated, but it became repeatedly turbid, nor could I by means either of the filter or alcohol prevent a recurrence of the fame effect. Nearly the fame refult takes place when carbonate of ammonia is ufed as the precipitant.

Some of the white fcales were moiftened with fulphuric acid. No vapour arofe.

Some of the precipitate obtained by means of carbonate of potafh from the watery folution of this fubftance, was, after fufficient edulcoration, diffolved in fulphuric acid; the folution, on due evaporation, produced permanent cryftals, fome of which refembled alum, but others feemed to differ from it in external character. Ammonia decompofed the folution of them in water, and a few drops of liquid potafh diffolved the precipitated earth. The quantity was too fmall for further experiment.

If diftilled water be poured into the retort and boiled in it, fo as to diffolve what adheres to the neck and cavity of it, a further folution is effected, but differing in fome meafure from the folution of the fublimate collected from the neck of the veffel. This latter folution is found to contain lead. If nitric or muriatic acid be poured into the retort, fo as to diffolve what *ftill* remains adhering to it, the prefence of lead becomes more evident. Whence does this metal arife? I have reafon to believe that it arifes from the glafs retort, which is corroded by the acid of the foffil extricated by heat. But what acid is it? It does not feem to be either the phofphoric or fluoric acids, the latter of which became the firft object of my fufpicion.

The opinion which Mr. Davy fuggefted to me feems more probable, that it is of vegetable origin. Oxalic acid, on the authority of Bergman, may be volatilized; yet fome of its properties are very extraordinary and do not accord with this idea.

I decompofed the watery folution of the fcales by nitrate of lead, and after a fufficient edulcoration of the fubfided precipi-

tate, I dropped upon it fome fulphuric acid. No fumes were perceptible. The fulphate of lead was feparated by the filter, and the clear fluid, which paffed through it, was gradually evaporated; fmall cryftallizations were formed, the figure of which I could not afcertain; fome of them were expofed to the flame of the blowpipe in a gold fpoon; they did not burn to coal, nor give out any empyreumatic fmell nor fufe, but they affumed an earthy appearance *.

Defcription and analyfis of a mineral from Cornwall.

Uran-glimmer.

I fhall add a few defultory remarks upon the yellow and green cryftals, which frequently accompany the foffil.

I confidered them to be the two fpecies of uran-glimmer which had been examined by the celebrated Klaproth.

The yellow cubic cryftals are light. Their fpecific gravity, taken at temperature 45° Fahrenheit, was 2,19.

Expofed to the flame of the blowpipe on charcoal, they decrepitate violently. A piece of this fubftance is taken up by phofphate of ammonia and foda, without effervefcence, and communicates a light emerald-green colour to the fufed globule.

By expofure to a red heat, this fubftance lofes nearly a third part of its weight. It then becomes of a braffy colour.

It is foluble in the nitric and muriatic acids; but I could procure no cryftallized falt from the folution of either of them.

By evaporation to drynefs, and rediffolving the mafs, fome filica is feparated.

A

(1.) A certain quantity of the yellow cryftals were diffolved in nitric acid. Muriatic and fulphuric acids fucceffively dropped into the folution produced no fenfible change. The contents of the folution were precipitated by ammonia,

* I fubjected fome of the Barnftaple mineral, with which Mr. Rafhleigh kindly furnifhed me out of his cabinet, to experiment, with a view of afcertaining whether it would produce the fame volatilized faline cruft, as the ftenna gwyn foffil, and I found that it did.

in

Description and analysis of a mineral from Cornwall.

in white clots, mixed with some of a yellowish hue. Ammonia, added in excess, betrayed no sign of the presence of copper.

(2.) The ammonia, on evaporation, was found to have held a portion of the mineral in solution. A fresh portion of ammonia dissolved more, but in a less quantity, at each succeeding affusion of it.

(3.) The precipitate, which had resisted the ammonia, was boiled in a silver crucible, with a solution of potash in alcohol, diluted with distilled water, and a considerable portion of the substance was dissolved by it: the potash and the ammonia had dissolved rather more than half of the fixed ingredients of it.

(4.) The edulcorated residuum, which was of a dirty yellow colour, was transferred to a crucible of platina, and moistened with sulphuric acid, which was abstracted from it, in the sand-bath. The brownish-gray mass was elixated with distilled water, which dissolved nearly the whole of it. The residuum consisted of a white heavy powder, which, tried in different ways, was found to be *sulphate of lead.*

(5.) The solution effected by sulphuric acid was greenish. On evaporation, a salt was produced, of uncommon brilliancy, resembling scales of mica, or silver leaf. These diminished in quantity at every fresh solution and evaporation, and at last they could not be reproduced; but a confused crystallized mass remained. How far the platina crucible may have contributed to this phenomenon I cannot ascertain.

(6.) The solution of the saline mass was precipitated by potash, of a dark brown colour. The potash held nothing in solution. I redissolved the precipitate in nitric acid, and precipitated the solution by ammonia, of a bright yellow colour, peculiar to the *oxide of uranium*, with which it agreed in other properties.

(7.) What was dissolved by ammonia (2) amounted to nearly $\frac{1}{8}$ part of the fixed ingredients. It was white, inclining to ash-colour. It tinged phosphate of soda and ammonia of a light green It was soluble in sulphuric acid, except a few gelatinous flakes. The solution was greenish; gradually evaporated, it shot into a number of minute stellated crystallizations, which were circular, and consisted of rays diverging from a centre. They were, in general, colourless: a few of

them

them were tinged of a smoke-colour. They soon became deliquescent. Upon evaporation, the same crystallizations were produced. After a time, some detached, regular, and permanent crystals were formed, which were colourless. Their figure I could not accurately ascertain. They were exposed to a red heat in a platina crucible. No ammoniacal vapour was perceptible. The crystals melted into opaque globules; some of these were transferred to a small glass, and distilled water was poured upon them. No solution took place apparently: on shaking the glass, the globules fell to pieces into gelatinous flakes, which were white. Some of the supernatant fluid was tried with muriate of barytes, which produced a cloud. But neither ammonia nor prussiate of potash caused any change in it. It is soluble also in nitric acid: the solution formed a confused crystallized mass, which soon became deliquescent. Zinc, immersed in it, caused the separation of white gelatinous flakes. Iron caused no change. Ammonia and potash threw down white precipitates, a portion of which were redissolved. The carbonates of soda, potash, and ammonia produced white precipitates. Prussiate of potash threw down the contents of the solution in distinct flakes, of the colour of mahogany; and the solution of galls in alcohol caused a light yellow powder to subside. It is soluble also in muriatic acid; the solution is a very dilute green. It requires an excess of acid to hold the substance in solution: which, after a time, deposits crystalline grains of a yellowish colour, which require a large quantity of water to dissolve them.

Description and analysis of a mineral from Cornwall.

Acetic acid does not dissolve this powder.

(8.) What was dissolved by potash (3.) was of an isabella colour; it was tried with nitric, muriatic, and sulphuric acids, neither of which could dissolve the whole of it. What resisted the two former acids was found to be silica. That which remained undissolved by the latter, was silica and sulphate of lead. Evaporation of the latter solution, betrayed also the presence of lime, in the state of sulphate. The nitric and muriatic solutions, on evaporation, deposited nitrate and muriate of lead; and sulphuric acid dropped into them produced a small quantity of sulphate of lime.

The nitrate and muriate of lead were decomposed by sulphuric acid, and the lead reduced on charcoal.

Ammonia

Description and analysis of a mineral from Cornwall.

Ammonia precipitated what remained in these solutions, and redissolved a part of the precipitates, which agreed in properties with that substance before mentioned (2.); the remainder was of a brighter yellow. But I could not bring the solution of it in nitric acid to crystallize.

B.

(1.) Some of the yellow crystals, which had not the slightest appearance of being contaminated with extraneous matter, were dissolved in sulphuric acid. *Silica was separated;* and the presence of *lime* and *lead* proved by the appearance of their respective *sulphates*.

(2.) If sulphate of ammonia is dropped into a solution of this mineral in nitric or muriatic acids, no change takes place, *immediately*. But on evaporation, a yellowish crust is deposited, which is insoluble in water. A solution of carbonate of soda in water, boiled on it, becomes yellowish-brown, and the greater part of it is dissolved. The residuum, which is white, is reduced on charcoal to a globule of lead. What the carbonate of soda had dissolved was found to be *oxide of uranium*. Sulphuric acid *alone*, does not produce this deposited crust.

(3.) Some perfectly pure crystals were dissolved in muriatic acid. Some silica was separated. A few drops of sulphuric acid were dropped into the solution, which produced no immediate change: on evaporation a white powder, separated, which consisted in part of sulphate of lime. The remainder, exposed to the flame of the blowpipe, was reduced to globules of lead.

The solution was decomposed by ammonia, which redissolved a part of the precipitate; and, after edulcoration, the precipitate was dissolved by nitric acid, and precipitated again by ammonia, which held a less quantity in solution. The edulcorated precipitate was now boiled with a solution of carbonate of soda, which dissolved a large portion of it. The solution was yellowish-brown, and contained oxide of uranium. What was undissolved by the carbonate of soda was dissolved in sulphuric acid, and seemed to be the same substance as that which the ammonia held in solution, A. (2.)

The

The scarcity of this beautiful mineral has precluded me from operating on such a sufficient quantity, as a regular and rigid analysis required.

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The substance, which is held in solution by ammonia, has some peculiar properties that seem to distinguish it from uranium. And if this mineral be the uran-glimmer, I have certainly detected the oxide of lead, lime, and silica in it, which have not hitherto been considered as ingredients of that fossil. The green crystals differ in no respect from the yellow, except in containing a little of the oxide of copper.

XVII.

Examination of different Methods of separating Nickel from Cobalt. By M. C. F. Bucholz.*

THE want of nickel and cobalt in a state of purity induced M. Bucholz, to make experiments himself on the means of procuring them, and to repeat those of others.

A. The able chemist Hermstadt proposed to separate oxide of cobalt and oxide of nickel, by dissolving the nitrate or sulphate of cobalt, impregnated with nickel, in ammonia, and exposing the solution to a single evaporation. This M. Bucholz tried in the following manner, for the reversed purpose.

M. Bucholz repeats Hermstadt's method.

1. An ounce of cobalt ore (*cobalt speise*) was dissolved with heat in four ounces of nitric acid of the specific gravity 1,220, and mixed with an equal quantity of water; which produced a residue of three drams of oxide of arsenic, in the form of small crystals. When the solution mixed with half the quantity of water, coloured of a dull green, had been filtered and diluted with a great quantity of water, it deposited a little of the oxide of bismuth. Caustic ammonia was then mixed with it to excess, until no farther apparent solution took place of the precipitate obtained. That which was not dissolved, of a dull reddish white, was a composition of arseniate of cobalt with a little of the oxide of bismuth, and the oxide of iron.

One oz. cobalt ore dissolved in nitric acid.

Deposits 3 drams of arsenic.

The solution filtered and diluted deposits a little bismuth oxide.

Caustic ammonia added.

The undissolved residue is arseniate of cobalt, with oxides of bismuth and

* Bucholz, &c. Journal of Chem. III. p. 2.

The

By evaporation oxides of cobalt and nickel are precipitated.

The solution being filtered, appeared of a beautiful blue, it was then evaporated at a gentle heat, by which about two drams of a bright green precipitate were obtained; which proved to be oxide of nickel, united to oxide of cobalt. The filtered liquor being then afterwards evaporated at the heat of a stove, deposited still an oxide of the same quality.

The saline mass obtained, re-dissolved, filtered, and boiled with caustic potash produces pure oxide of nickel.

The saline mass of ammoniacal nitrate of nickel, of a deep green colour, which had been oblainted by the evaporation, was re-dissolved, filtered, and kept in ebullition with an excess of caustic potash, until the evaporation of the ammonia was compleated, by means of which a dram and half of oxide of nickel was separated, which did not appear to contain any more oxide of cobalt.

Gives an odour of oximuriatic acid.

2. As the separation was not effected very well nor with much facility by the former method, the effect of sulphuric acid was tried. For this purpose, an equal quantity of water was poured on the oxide obtained as before, and sulphuric acid added till all was dissolved by the aid of heat. It then evidently gave out an odour similar to that of oximuriatic acid, although there was not any muriatic acid used. A like phenomenon, on a similar occasion, was before observed by the author (which is mentioned in the first section, page 18, of *Deitrage zur erweiterung*, for 1799.) The solution was then treated with ammonia as before, until the whole was almost dissolved. The residue, which was oxide of cobalt with a little oxide of nickel, had the colour of verdigris, When the solution was evaporated at a gradual fire, and separated by filtration from the precipitate, of which the greatest part was oxide of cobalt, it was submitted to spontaneous evaporation: It then crystalized without any farther separation, partly into prismatic crystals in groups, and of a green colour, and partly into crusts united together, and blue at the edges; the essay of the oxides procured by potash from the solution of the crystals, as well as from the mother water, shewed that they contained cobalt almost in equal proportions,

Treated with ammonia deposit oxide of cobalt.

The solution crystalised.

The crystals contain cobalt and nickel.

The last experiment repeated on a larger scale

3. Mr. Bucholz repeated the former experiments on a larger scale, in hope to obtain a better crystallization, and operated on eight ounces of cobalt ore, from which the first crystals, of a blueish green, obtained by a process similar to that last recited, and which weighed about five ounces, were again dissolved in 32 ounces of boiling water: This solution was evaporated

porated till a pellicle was formed, and, after being filtered, was left near a stove, that it might cool slowly and crystallize. At the end of 48 hours, the greatest part of the salt was crystallized in beautiful tetrahedral rhomboidal pyramids, short, and of a yellow green, of which the lateral faces formed an angle of 45 and of 66 degrees, often with one extremity truncated, and always with an angle of 132 degrees towards its terminating face. This result proves that this salt forms more readily into regular crystals by cooling than by slow evaporation. All the crystals were then collected, washed with water, and again dissolved, and the nickel separated by boiling the solution with potash till the ammonia was disengaged.

Produces fine crystals in tetrahedral rhomboidal pyramids.

The crystals dissolved and the nickel separated as before.

4. As well to free this oxide from carbonic acid as to judge if it had been purified from cobalt, it was dissolved in nitric acid and treated with pure ammonia in the same manner as has been described. The liquor of a fine blue colour, (and from which a residue of five grains, which seemed to be an oxide of cobalt, had been separated by filtration), was evaporated to dryness. After another solution then made, it deposited an oxide of a beautiful bright green, which, after being washed and dried, weighed half an ounce. The liquor, which passed the filter, was analysed by pure carbonate of potash at the heat of boiling water, which then produced 170 grains of oxide of nickel, of a pale green, united to carbonic acid; a little of it was dissolved in muriatic acid, and some of the solution spread upon paper. On heating it afterwards, the tint became yellow, and inclined but very little to a green. But the oxide of nickel, which separated spontaneously during the evaporation, was dissolved in disengaging much oximuriatic acid; spread on paper, it exhibited the colour, when heated, of a sympathetic ink of cobalt highly saturated; from whence it follows that it was more rich in cobalt than that procured from the precipitation.

The oxide obtained dissolved in nitric acid and treated with ammonia, evaporated and re dissolved deposits a green oxide.

The filtered liquor yields by potash 170 grs. oxide of nickel.

Which contains less cobalt than the spontaneous precipitate.

The oxides collected in those two ways, dissolved in nitric and sulphuric acids, after becoming grey, (which the author supposed to be occasioned by the nickel dissolving first, and at least the greatest part of the cobalt remaining to the last, but which opinion was not confirmed by other experiments made on this subject.) These oxides made lightly red in the fire, changed their colour to a dark grey, and then, as well as on

The oxides dissolve in nitric and sulphuric acids.

Give out nitrous acid by fire, and by sulphuric acid.

on the addition of fulphuric acid, a difengagement of nitrous acid took place from the refidue obtained by evaporation, which was alfo caufed by the addition of an alkaline lixivium: With ammonia the fame effects were produced which have been before mentioned.

Sulphites and nitrates of ammoniacal nickel always contain cobalt.

The refults of the foregoing experiments are:—The fulphates and nitrates of ammoniacal nickel feparated from cobalt ore, retain always fome cobalt in their compofition, and it is impoffible from the method of Hermftadt modified in the preceding manner, to obtain an oxide of nickel without a mixture of cobalt.

The oxide of nickel remaining in the falt after evaporation contains very little cobalt.

b. By partially decompofing the ammoniacal nitrate of cobalt by evaporation, an oxide of nickel is obtained, very rich in cobalt, which contains nitric acid; and the oxide of nickel which remains undecompofed in this falt, retains a very fmall quantity of cobalt.

Dr. Schnaubert's method of obtaining pure oxide of nickel.

B. Doctor Schnaubert has publifhed (in Tromfdorf's Journal of Pharmacy, vol. II. p. 66) a method of obtaining the oxide of nickel pure: Which confifts in diffolving the metal of nickel mixed with cobalt, or its oxide feparated from other fubftances, in nitric acid, in precipitating it by the carbonate of potafh, and in heating it to a white heat, after wafhing and drying it. In this manner he always procured a yellow oxide, on which he caufed very ftrong fulphuric acid to boil; which gave him a folution of oxide of nickel of a grafs green, while the oxide of cobalt appeared in the form of a yellow refidue. He proves the purity of the fulphate of nickel prepared in this manner, by the property which ammonia has of precipitating it of a bright green, and when added to excefs, of re-diffolving it with a beautiful deep blue colour; but this argument appears infufficient to thofe who know that oxide of nickel, although mixed with many hundredth parts of cobalt, does not, however, experience any perceptible change in the colour of its precipitates, nor in its ammoniacal folutions.

His teft of its purity defective.

He has not mentioned his proof that the other oxide obtained was cobalt,—nor the degree of heat to be ufed,—nor the ftrength of the fulphuric acid employed.

Befides the omiffion of indicating the means by which he was convinced that the oxide, which was the refidue of the fulphuric acid folution, was really an oxide of cobalt, with the vague precept of heating the oxide acquired, without the leaft direction relative to the degree of the fire, and the uncertainty which he leaves of the degree of ftrength of the

fulphuric

ſulphuric acid which he uſed, altogether throw doubts on the exactneſs of the proceſs indicated, which the following experiments may elucidate.

1. A portion of the carbonic oxide of nickel, A 1, was exposed during an hour in a ſtrong fire to a red heat approaching white heat. The oxide while hot was of a browniſh yellow; after cooling it aſſumed a grey colour inclining to yellow, but not yellow. The oxide obtained by the evaporation A 4, having been treated in the ſame manner was ſtill a little more grey than the preceding. The carbonic oxide of nickel was placed again for half an hour in a white heat; while hot it was yellow inclining to brown, but when cool, it was grey inclining to browniſh yellow.

Experiment on Dr. Schnaubert's proceſs.

2. Thirty grains of this oxide made red (hot), were put for ſome hours to digeſt, with ninety grains of pure ſulphuric acid of the ſpecific gravity 1,860. Being then heated, the maſs immediately ſwelled up with an exploſive noiſe, and exhibited a yellow ſubſtance inclining to a green; by means of ebullition with half an ounce of water it was diſſolved, except about a grain of a yellowiſh-grey powder, which proved to be an oxide of nickel mixed with cobalt and a little dirt. Thirty-five grains of oxide of nickel, (obtained by heating briſkly to redneſs 60 grains of ammonical nitrate of nickel prepared by evaporation), afforded the ſame reſult, and the ſame phenomena, on being treated in the ſame manner: The ſame oxide being heated for half an hour to whiteneſs, uſing the bellows at the ſame time, did not afford a yellow maſs, but one of a yellowiſh grey inclining to a green, which had the ſame effect with ſulphuric acid that has been already related.

3. The experiment was again repeated with diluted ſulphuric acid; 160 grains of ammonical oxide of nickel, which had been precipitated from many ſolutions were expoſed for half an hour to the moſt violent white heat, under the operation of the bellows, after which they weighed 75 grains. This ſubſtance was of a greeniſh yellow here and there, and of a blueiſh grey where it touched the crucible; being broken it produced a black grey powder. It was mixed with a dram of ſulphuric acid diluted with five drams of water; at that inſtant there was a rapid diſengagement of gas, and on heating the mixture it evidently gave out hydrogen gas. After a ſuffi-

The experiment repeated with diluted ſulphuric acid.

cient ebullition, water was added, and the solution decanted off clear. The residue was treated again with weak sulphuric acid, and then gave a residue of ten grains which was by no means oxide of cobalt, but oxide of nickel mixed with cobalt, as its solutions in the acids and in ammonia proved. The two preceeding solutions were each separately analysed by pure potash, and the precipitate was besides heated with an excess of potash, and then washed and dried. At the proof each of the precipitates afforded cobalt, which was always most pure in that of the first solution; for the solution in muriatic acid, laid on paper, and heated, inclined perceptibly to a yellow, while the precipitate of the second solution produced a stain of a clear and pure green. It is strange that the first solution afforded more oxi-muriatic acid than the second.

The oxide of [illegible] mixed with [illegible]de of

E[illegible] of the pre[illegible] tes of the [illegible]ing solu[illegible] afford cobalt.

These experiments, and others made by the author; but not related, prove,

The experiments prove that nickel oxide does not become yell[illegible]; cause of the [illegible] of Dr. [illegible] on [illegible].

A. That the oxide of nickel heated either slightly or violently does not assume a yellow colour; and if this colour was observed by M. Schnaubert, it must have been caused by some substances which entered into the composition of the oxide, or perhaps by the mixture of a little arsenic.

It is [illegible] to o[illegible] pure nick[illegible] his way

B. That it is impossible by M. Schnaubert's method, to obtain an oxide of nickel exempt from cobalt; since it does not even effect a separation of the two oxides so far as to be perceptible to the eye.

M. [illegible]man's me[illegible] too tr[illegible] and ex[illegible], and M. [illegible]gman's also

C. M. Bucholz hints here at several experiments he made with a view to find an acid which would form an insoluble salt with one of the oxides, and one easy of solution with the other, but which, as they did not succeed, he does not mention; and as the method proposed by Mr. Lehman (in the *Cadmiologia*, part II, page 110) of fusing fifteen or twenty times, to a commencement of vitrification, a mixture of nickel and cobalt, in order to scorify all the cobalt, would be too troublesome and expensive, as would that also indicated by Bergman (*Opuscul, Physic, et chem*, Vol. II. p. 246—249) of repeating the fusion three or four times with from 8 to 12 times the quantity of pure nitre. The process indicated A 4, (consisting of a partial decomposition of the ammonical nitrate of nickel), alone remained to be repeated. For this purpose

The process A 4 repeated.

oxide

oxide of nickel, (which was separated from the triple salt, not dissolved at the first evaporation, by carbonate of potash, was treated repeatedly, (in such a manner) that after dissolving it in nitric acid, recourse was had to the use of ammonia and evaporation as before described. In this method was obtained, entirely free from cobalt, an oxide separated by potash from the triple salt, which had been redissolved after evaporation, and which oxide had the properties mentioned in the memoir printed in the second volume of the Annales de Chimie.

The oxide which was separated by evaporation from the ammoniacal nitrate of nickel, was in the last operation entirely freed from cobalt; it only contained a still, as has been observed, a little nitric acid. The oxide of nickel, which, after having been laid bare by evaporation, still contains cobalt, may naturally undergo the same operation over again.

The oxide entirely freed from cobalt in the last operation.

This method may be made use of untill one more expeditious is discovered by farther experiments, since it does not occasion any considerable expence, for by potash, the evaporation of the ammonical nitrate of nickel may be effected in a retort, and also the subsequent decomposition of the triple salt, and thus the ammonia may be separated for other uses; in like manner, in works on a great scale, a part of the nitre may be recovered from the last operation, by the evaporation of the water in which the substance has been washed.

This method recommended for the present, the ammonia may be saved during the process, and the nitre recovered.

XVIII

Sugar prepared from Beets. By M. Hermbstadt.*

THE method of M. Achard for extracting sugar from beets, was so expensive, that it was of no advantage for common use. M. Hermbstadt, of Berlin, has practised another method, which is easily performed, and affords hopes of rendering this sugar cheaper than that from the sugar-cane; which is as follows:

After having bruised the beets in a mortar, M. Hermbstadt submits them to the operation of a press, to extract the juice from them; which is then placed in vessels, and clarified with lime in the same manner as cane-sugar.

The expressed juice of the beet is clarified by lime,

* Sonnini's Journal, Tom. II. p. 331.

When

and then evaporated to a syrup. A coarse sugar is produced on cooling.

When this operation is finished, the liquor is evaporated to the consistence of a syrup: It is then left to cool, and a coarse sugar is obtained, of a dark-brown colour: At the bottom of the vessel a syrup remains, which may be used for domestic purposes.

From 100 lb. of this coarse sugar, 80 lb. of refined sugar may be obtained.

From 100 pounds of the coarse sugar, eighty pounds of well crystallized sugar are obtained by the first refining, which sugar is not at all inferior in quality or whiteness to that of the cane. The whole operation may be completed in two days.

It is probable M. Hermbstadt used the common field beet, or root of scarcity.

The particular species of beet which M. Hermbstadt used in his experiments, is not mentioned; but it is most probable that this chemist made use of the common field beet, known in Germany by the name of *mangel wortzel*, the culture of which is spread through many cantons of Germany. This variety, however, contains less sugar than all the other species of beets; and, for this reason, M. Sonnini is of opinion, that if sugar can be obtained from beets with profit and economy, more success would be obtained by submitting to the operations described, the small red beet, called in France that of *Castlenaudery*, which is the sweetest of all.

A better produce might be obtained from the small red beet of Castlenaudery.

XIX.

Method of stacking Turnips, to preserve them through the Winter. By Mr. JOHN SHIRREFF, *of Captain Head, near Haddington, N. Britain.**

Rapa solo molli et aere humidulo lætantur.

Preservation of turnips through the winter.

SATISFIED, from observation and experience, that turnips are the foundation of the best husbandry on almost all soils and situations in the arable districts of Great Britain; and that this crop should always be drawn, except from blowing sands, or light moorish soil, on both of which it should always be in part consumed on the ground with sheep; convinced also, that turnips, if possible, should be off all soils, and the land

* Soc. Arts, 1805. The premium of 30 guineas was awarded for this method.

ploughed

ploughed up before the middle of December, at the lateſt, to ſecure the ſucceeding corn crop, and graſſes, or clovers, with either of which every field that carried a turnip crop the preceding ſeaſon, ſhould, in almoſt every caſe, be ſown down; and impreſſed with the many high advantages attending this practice, as ſoon as my pea and bean ſtubbles are ploughed up, and ſown with wheat, my turnips are begun to be drawn, and ſtacked up for uſe during the following winter and ſpring. If the diſtance of the turnip-field from the homeſtead does not exceed a quarter of a mile, two double horſe carts only are employed, and more in proportion to the diſtance of the turnip field, or number of hands you may be able to command to carry on the work. One clever driver is ſufficient for two carts, and two for three carts, &c. one cart being always in the field loading or loaded. On being brought home, the turnips are inſtantly tumbled out at the ſtack; which is done with great facility, from the conſtruction of the carts in this diſtrict, which to convenience and ſtrength likewiſe add lightneſs, to enable horſes to move at a ſmart pace with them when empty. The turnips tumbled out of the cart, are trimmed of their leaves, and cleaned of any earth that may adhere to them, by women, &c. before being put into the ſtack. Old table-knives do very well for the purpoſe, and the leaves ſhould be cut off cloſe to the root; the back of the knife being uſed for removing any pieces of ſoil that may ſtick on the turnip.

Preſervation of turnips through the winter.

Women, &c. trim the turnips, and put them into ſtrong coarſe wicker baſkets, to be carried forward by a man, who hands them to another, who lays them into or on the ſtack. The ground on which the turnips are placed ought to be dry bottomed. If that is not the ſort of ſoil where you find it moſt convenient to make your ſtack, a quantity of boulders may be put on, regularly ſpread over the ſpace, to the thickneſs of at leaſt eighteen inches. My corn-rick yard, being dry ground, has been uſed as the place for keeping my turnips in. The ſtacks have been made about ten feet wide, by driving a row of ſtakes into the ground parallel to the wall of the yard, which ſerves inſtead of another row. The wall is only about five feet and a half high, and the ſtakes are driven to the ſame height. The inſide of the wall and ſtakes are lined with compact bunches, or ſheaves of wheat-

 ſtraw,

Preservation of turnips through the winter.

ſtraw, about ten inches in diameter, placed horizontally on the ground or boulders, and introduced, as wanted, during the operation of ſtacking. A tire of the largeſt turnips are placed one above another, on the inſide of the bundles of ſtraw, more particularly on the ſide guarded by the ſtakes, till the pile reaches the height of five feet from the ground, or from the boulders, if it has been found neceſſary to ſpread any over the ground. The inner part of the ſtack is at the ſame time gradually made up with turnips put in promiſcuouſly; along which a plank is laid, and occaſionally ſhifted as the pile riſes, for the man who builds the ſtack to ſtand on without bruiſing the turnips with his ſhoes. When the pile of turnips is reared, in the manner deſcribed, to the height of above five feet, it is gradually contracted inwards, on both ſides, at an angle of about forty-five degrees, like the roof of a barn; the largeſt turnips being ſtill piled on the outſide, till the roof is ſo far completed. The ſtack is every day ſo far finiſhed in height as it is extended in length, and is covered with wheat ſtraw thatch, roped down with twiſted bands of oat-ſtraw before evening, to ſecure the ſtacked turnips from rain that may fall during the night. The thatch is laid on a foot thick, and ſecured in the ſame ſimple, effectual manner, that corn-ricks are covered in Northumberland, Berwickſhire, and the Lothians; with this difference only, that the ſtraw is four times as thick laid on the turnip as on the corn, to exclude cold as well as wet; and that there is a rail of wood ſtretched, hanging horizontally at the tops of the wall and ſtakes, to fix the ſtraw ropes to, which ſecure the thatch on the ſtack. The end of the ſtack is every night covered with bundles of wheat-ſtraw, which are removed next day, or when building recommences.

Three men are employed in the field to load and diſpatch the carts, occaſionally aſſiſting four women who draw the turnips, ſtriking off the top root with a ſtrong heavy knife, leaving the turnips on the tops of the drills as drawn and chopped, with the leaves all in one direction, to be readily laid hold of by the men who lift them up to the cart. The horſes paſs along in the ſpace between the two rows or drills of the turnips, which may be drawn: and, being at thirty inches apart, and the extremities of the wheels about five feet from each other, it is evident a wheel runs in the middle

of

of each ſpace between the contiguous drills, without injuring the turnip, whether drawn or not. When the cart is about to turn, after being loaded, the men move the turnips to make room for the horſes, putting them into the cart as part of the load.

Preſervation of turnips through the winter.

Expences of drawing, carting, trimming, ſtacking, covering, &c. a ſtatute acre of good turnip,—at the diſtance of not more than a quarter of a mile from the ſtack.

	£.	s.	d.
Two double-horſe carts, and one man	0	16	0
Two men loading, drawing, building, &c.	0	8	4
Seven women drawing and trimming	0	4	1
Two girls trimming	0	1	0
Four ditto and boys ditto	0	1	8
Twiſting ropes, drawing thatch, thatching, waſte of thatch, ſtakes, &c. ſay	0	3	6
	1	14	7

The above is a fair average of the expence of ſecuring ſomewhat more than twelve and a quarter ſtatute acres laſt ſeaſon, which was all I drew; and one field of two acres, one rood, thirty-three perches, was ſo far diſtant as to require three carts, and two drivers. That field, however, was firſt drawn, and the weather being fine and moderate, more work was done in proportion to the length of the day, which was alſo longer. Women and children cannot, indeed, exert themſelves with ſpirit, in raw cold weather. October is perhaps the beſt month to draw in. It is a queſtion with me, whether the average of the acres that are under turnip in the iſland, if the weight exceeds twenty-four tons, does not coſt more, merely for drawing and carting only. When it is conſidered that this operation is performed often in cold, froſty, and ſtormy weather, and that frequently much ſnow may be to be removed before the turnip can be ſeen. If no ſnow has fallen before the froſt ſets in, the turnips muſt be hoed up with inſtruments for the purpoſe. Many are cut, and much left in the ground of the lower part of the root. After all this labour, what is obtained is frequently no better than a lump of ice, environed with earth, frozen ſo firmly to its ſurface, that nothing but thawing in

 cold

Preservation of turnips through the winter.

cold water can ever render it fit to be touched by the mouth of any animal whatever.

Admitting, however, the expence of drawing and carting to be the fame, all that can be ftated as extraordinary expence is the coft of trimming and facking, which amounts to 11s. 3d. an acre. On the other hand, we have the advantage of having fine frefh clean turnips, always fecure and at command, to carry on feeding and breeding ftock; at the fame time that all lofs by rotting in the fpring months is prevented, which is frequently thirty and even fifty per cent. on all the crop that remains in the field, after the firft of February. Above all, the practice of drawing and ftacking before winter, by admitting of early ploughing to mellow the foil, fecures a valuable corn, and fucceeding clover crops. When all thefe circumftances are maturely weighed, the expence of eleven fhillings and three-pence will, to every enlightened agriculturift, appear but trifling to obtain fuch very valuable advantages. The writer of this little effay has had the fatisfaction of having excellent crops after his turnips, this feafon; while almoft every other crop in the neighbourhood was indifferent; and fome on rich dry loams, high rented, by being fown in the months of April and May, on the fpring ploughing, after turnips eaten off with fheep, were fo miferable, as evidently to pay *nothing* after expences of labour, feed, and reaping. The young clovers too, fown with thefe crops, have almoft entirely perifhed from want of moifture. The lofs of the crop and clover feed is not all: the fyftem fuffers a derangement, the confequences of which none but practical men can calculate.

One thing remains to be noticed, which is, that twenty-fix young cattle, cows, and yearling calves, were kept nearly three weeks on the turnip trimmings, with oat-ftraw along with them, to their improvement; and that many more might have been kept, had they been provided in time. A quantity of good manure was made: and, eftimating all advantages arifing from the confumption of the leaves in this way, at no more than 3d. a head per night, for the keep of each beaft, the amount will exceed the expence of trimming and ftacking the whole crop of turnips on twelve acres and a quarter.—The leaves that remain on turnips after Chriftmas, are either unfit to be eaten, or wafted by the frofts.

T. SHIRREFF.

XX.

Account of some Specimens of Basaltes from the northern Coast of Antrim. By the Rev. Dr. William Richardson.*

The Reverend Dr. William Richardson, late F. T. C. D. having sent to Dr. Hope a collection of specimens from the northern coast of Antrim, with a catalogue and observations, the specimens were exhibited, and the observations were read in the Royal Society, March 1803.

Remarks on the basaltes of the coast of Antrim.

Siliceous Basalt.

Dr. Richardson discovered the fossil to which he gives this name, in the peninsula of Portrush, four or five years ago. It abounds also in the Skerry islands, a reef of rocky islots extending from the northern point of Portrush-head for about a mile eastward. A small part of every one of those islots is formed of this stone, while the remainder consists of coarse basalt, similar in all respects to that on the east side of the above-mentioned peninsula. It is met with in one or two other places.

This stone is arranged in strata, from ten to twenty inches thick, all steadily parallel to one another, and every stratum, as far as can be observed, preserving an uniform thickness through its whole extent. When these strata are quarried into, they appear to be constructed of large prisms, generally pentagonal, which when broken divide into smaller prisms. This internal prismatic construction frequently gives an irregular or shivery appearance to the fracture, which however is often conchoidal, and the grain as uniform as in the Giant's Causeway basaltes.

The beds of this fossil are remarkable for containing marine exuviæ in great abundance, particularly impressions of *cornua ammonis*. The flat shells and impressions contained in these stones, are steadily parallel to each other, and perpendicular to the axis of the prisms. It must be observed, that the prismatic construction is never interrupted by the shells dispersed through it; the planes which separate the prisms passing equally through the shells and the stone itself.

* Edinburgh Transf. Vol. V.

The

Remarks on the basaltes of the coast of An-

The grain of this stone passes by insensible shades from a high degree of fineness, until it become undistinguishable from that of the common columnar basaltes.

The name of Siliceous Basalt, which Dr. Richardson employs, was first given to this fossil by Mr. Pictet of Geneva, when he visited Portrush, in a tour through Ireland two years ago. He considered it as a variety of basalt, containing a greater proportion of *silica* than usual.

The strata of siliceous basalt, both at Portrush and the Skerry islands, generally alternate with strata of equal thickness of a coarse-grained basalt of a grey colour. The materials of the strata grow into each other, so as to form one solid mass, from which it is easy to quarry pieces in the confine of the two strata, with a part of each adhering; but the coarse basalt, as it approaches very near to the fine, always abates somewhat of its coarseness; yet the line of demarcation is left completely distinct.

(The conclusion in our next.)

SCIENTIFIC NEWS.

Almanack printed at Constantinople.

Almanack printed at Constantinople.

FOR the first time an almanack has been printed at Constantinople, under the direction of Abdorahman. The printing-office was established in 1716, by *Said* (who had been at Paris with his father, the ambassador), and by Ibrahim, an Hungarian: Achmet the Third patronized them, and they printed many books; but an almanack was never before printed.

Observatory at Bavaria.

Bavarian observatory.

The Elector of Bavaria, a few months before the arrival of the French armies, caused an observatory to be erected in the neighbourhood of Munich. The situation chosen for its construction, takes in an extensive horizon. Professor Seyfer, a celebrated astronomer of Gottingen, was nominated director of this establishment.

2

Establishments

Eſtabliſhments for Natural Philoſophy in the Ukraine.

The rich land-owners in the Ukraine and Volhinia, have contributed largely for the eſtabliſhment of Lyceums for teaching natural philoſophy, at Krzeminico, and at Winnica. The library and philoſophical apparatus of the King of Poland, have been purchaſed for this purpoſe. M. Sniadecki has received a ſum equal to 500*l.* to purchaſe teleſcopes and clocks; and no expence is to be ſpared in properly furniſhing the obſervatories with inſtruments. Eſtabliſhment for natural philoſophy in the Ukraine.

Obſervatory at Moſkow.

M. Goldbach, an able aſtronomer of Leipſic, has been nominated profeſſor of the univerſity of Moſkow, with a ſalary equal to 250*l.* He is to have the direction of the conſtruction of a new obſervatory, to furniſh it with inſtruments, to make regular obſervations, and to inſtruct ſome young men in practical aſtronomy who have been previouſly inſtructed in the preparatory ſciences, and to give a courſe of lectures in theoretical aſtronomy in one of the halls of the Univerſity. Obſervatory at Moſkow.

They poſſeſs many of Cary's teleſcopes, of different powers; an excellent aſtronomical clock; a chronometer, made by Arnold; a portable circle, of one foot diameter; and, it was reported, had ordered one of three feet diameter from the ſucceſſor of Mr. Ramſden: Thus M. Goldbach will be provided with every inſtrument neceſſary and uſeful to aſtronomy, at the obſervatory of Moſkow.

M. Goldbach has taken the opportunity of his journey, to determine the poſition of ſome towns; among others that of Riga, $1^h\ 27'.0$, and $56^\circ\ 57'.8$.

At the ſame time that M. Goldbach is engaged with the aſtronomical eſtabliſhment at Moſkow, MM. Schubert and Wiſniewſki are employed at the obſervatory of Peterſburgh; and there is reaſon to expect a ſeries of obſervations from that part of the world.

Solar Tables.

A ſet of tables of the ſun, compoſed by M. Delambre, have been printed at Paris, in which there are many new equations, Solar Tables.

tions, and of which all the elementary parts have been verified by new observations. A set of tables of the moon's motion are also to be printed, and when they are completed, those of the planets will follow. *

Bequest of Ernest the Second relative to his Observatory.

Ernest the Second's bequest to his observatory.

Ernest the Second, late Duke of Saxe-Gotha, was remarkably attached to astronomical studies. He made observations and calculations himself, assisted in composing books on the subject, and furnished the funds for their publication. He enabled M. Zach to measure a degree of the meridian in Germany, and defrayed the expences from his private purse; so that he united to the merit of a connoisseur in the science, that of an author, a patron, a man of science, and of a generous prince.

He left in his will a sum equal to about 1330*l.* to form a fund for the maintenance of the observatory of Seeberg, near Gotha, which was built out of his own private estate; and ordered his successor to erect no other monument to his fame, but the careful support of this establishment.

Baron de Zach, who has given a copy of the will in his Journal, adds, "That he can assure the lovers of science, that the will of the father will not only be fulfilled, but surpassed by his successor, the present Duke Emilius Leopold Augustus, who has already shewn the most marked proofs of his attachment to the sciences.

"In a codicil to the will the Duke repeated, 'I forbid expressly the elevation of any monument to my memory, or even an epitaph, or any monument at or near my tomb.'"

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

APRIL, 1806.

ARTICLE I.

Letter from T. YOUNG, *M.D. F.R.S. &c. claiming the Lamp described in our last Number, and demanding an Explanation from the anonymous Communicator.*

To Mr. NICHOLSON.

SIR,

I WAS much surprized on seeing, a few days ago, the figure of a lamp contained in the fourth plate of your Number for February last. I trust you will be convinced, upon inspection of the figure which I now send you, and which was engraved before Christmas, that your correspondent A. F. must have copied his lamp from that which is here represented; and I am sure you will think I have a right to demand a public explanation of the manner in which he procured a sight of a plate not yet published, and of the motives which induced him to make so unjustifiable a use of it. I shall reserve the complete explanation of this lamp for the work to which the plate belongs, which has been long in the press, and which will soon be ready for publication; I shall only observe that Concerning the invention of a Lamp.

it is in a great meafure free from the inconvenience which A. F. has attributed to it, (p. 168) and that the "fmall fhaded circle" is not a "perforation," but a weight attached to the counterpoife,

I am, Sir,

Your very obedient Servant,

THOMAS YOUNG.

Welbec Street,
March 15, 1806.

II.

On the Tendency of Elaftic Fluids to Diffufion through each other. By JOHN DALTON *.

Mixed elaftic fluids of different denfities do not feparate;

IN an early period of pneumatic chemiftry it was difcovered that elaftic fluids of different fpecific gravities being once diffufed through each other, do not of themfelves feparate, by long ftanding, in fuch manner as that the heavieft is found in the loweft place; but on the contrary, remain in a ftate of uniform and equal diffufion.

but will they mix without agitation.

Dr. Prieftley has given us a fection on this fubject (vid. Experiments and Obfervations, &c. abridged. Vol. II. p. 441) in which he has proved the fact above-mentioned in a fatisfactory manner; and every one's experience fince, as far as I know, has coincided with his conclufions. He has not offered any conjecture concerning the caufe of this deviation from the law obferved by inelaftic fluids; but he fuggefts that

Dr. Prieftley thinks not.

"if two kinds of air of very different fpecific gravities, were put into the fame veffel, with very great care, without the leaft agitation that might mix or blend them together, they might continue feparate, as with the fame care *wine* and *water* may be made to do."

The determination of this point, which feems at firft view but a trivial one, is of confiderable importance; as from it we may obtain a ftriking trait, either of the agreement or difagreement of elaftic and inelaftic fluids in their mutual action on each other.

* Manchefter Memoirs, Vol. I. New Series.

It

It is, therefore, the fubject of the following experiments to afcertain whether two elaftic fluids brought into contact, could intermix with each other, independently of agitation. The refult feems to give it in the affirmative beyond a doubt, contrary to the fuggeftion of Dr. Prieftley; and eftablifhes this remarkable fact, *that a lighter elaftic fluid cannot* reft *upon a heavier*, as is the cafe with liquids; but, they are conftantly active in diffufing themfelves through each other till an equilibrium is effected, and that without any regard to their fpecific gravity, except fo far as it accelerates or retards the effect, according to circumftances. Inquiry by experiment which fhews the contrary.

The only apparatus found neceffary was a few phials, and tubes with perforated corks; the tube moftly ufed was one ten inches long, and of $\frac{1}{20}$ inch bore; in fome cafes a tube of 30 inches in length and $\frac{1}{3}$ inch bore was ufed; the phials held the gafes that were fubjects of experiment and the tube formed the connection. In all cafes, the heavier gas was in the *under* phial, and the two were placed in a perpendicular pofition, and fuffered to remain fo during the experiment in a ftate of reft; thus circumftanced it is evident that the effect of agitation was fufficiently guarded againft; for, a tube almoft capillary and ten inches long, could not be inftrumental in progagating an intermixture from a momentary commotion at the commencement of each experiment. Apparatus.

FIRST CLASS.

Carbonic Acid Gas, with Atmofpheric Air, Hydrogenous, Azotic and Nitrous Gafes.

1. A pint phial filled with carbonic acid gas, the 30 inch tube and an ounce phial, the tube and fmall vial being filled with common air, were ufed at firft. In one hour the fmall phial was removed, and had acquired no fenfible quantity of acid gas, as appeared from agitating lime water in it. In three hours it had the acid gas in great plenty, inftantly making lime water milky. After this it was repeatedly removed in the fpace of half an hour, and never failed to exhibit figns of the acid gas. Things remaining juft the fame, the upper phial was filled with the different gafes mentioned above repeatedly, and in half an hour there was always found acid fufficient to make the phial $\frac{1}{4}$ filled with lime water quite Carbonic acid gas with lighter gafes.

 milky.

miky. There was not any perceptible difference whatever gas was in the upper phial *.

SECOND CLASS.

Hydrogenous Gas with Atmospheric Air and Oxigenous Gas.

Hydrogen, with atmospheric air and oxigen.

1. Two six ounce phials were connected by the tube of a tobacco pipe, three inches long, the upper containing hydrogenous gas, the lower atmospheric air: after standing two hours, the lower phial was examined; the mixed gases it contained made six explosions in a small phial. The gas in the upper also exploded.

2. Two four ounce phials connected with the ten inch small tube stood two days, having common air and hydrogen gas. Upon examination the upper was found to be $\frac{1}{3}$ common air by the test of nitrous gas. The gas in the under exploded smartly; that in the upper moderately with a lambent flame.

3. Two one ounce phials were connected by the ten inch tube, containing common air and hydrogenous gas; in three hours and a half the upper was about $\frac{1}{3}$ common air and the under $\frac{2}{3}$; the former exploded faintly; the latter smartly.

4. Two one ounce phials were connected as above; the under containing gas about $\frac{3}{4}$ oxygenous, the upper hydrogenous: In three hours the latter was $\frac{1}{5}$ oxygenous, and the former about $\frac{1}{2}$; the upper exploded violently, the under, moderately.

5. Two one ounce phials were again connected, the lower having atmospheric air, the upper hydrogenous gas; they stood fifteen hours, and were then examined; the upper gave 1.67 with nitrous gas, the under 1.66.—Hence it is evident that an equilibrium had taken place, or the two gases were uniformly diffused through each other in both phials.

THIRD CLASS.

Nitrous Gas, with Oxigenous Gas, Atmospheric Air, Hydrogenous and Azotic Gases.

Nitrous gas with oxigenous, atmospheric hydrogenous, and azote.

The results of the preceding experiments upon gases that have no known affinity for each other, were conformable to

* The small tube of ten inches was then used and a phial of common air; in one hour much acid gas had come through, as appeared by lime water.

what

what *à priori*, I had conceived; for, according to my hypothesis, every gas diffuses itself equably through any given space that may be assigned to it, and no other gas being in its way can *prevent*, though it may considerably *retard* this diffusion. But in some of the following experiments, in which the two gases are known to have a chemical affinity for each other, I expected different results from what are found; perhaps without sufficient reason. For, chemical union cannot take place till the particles are brought into contiguity; and the elastic force which sets them in motion appears, from the above experiments, to be a principle diametrically opposite to affinity. That circulation of elastic fluids, therefore, which we have now before us, cannot be *accelerated* by their having a chemical affinity for each other. Another circumstance deserves explanation;—when nitrous and oxygenous gas are in the two phials, the residuary gases after the experiment are nearly as pure as before; because those portions of them that meet in the tube, form nitrous acid vapour, which is absorbed by the moisture in the phials, and therefore does not contaminate either gas.

Nitrous gas with oxygenous, atmospheric hydrogenous, and azote.

1. Two one ounce phials were connected with the small tube, the under containing nitrous gas, the upper atmospheric air; after three hours, the upper phial was taken off when a quantity of air was perceived to enter, as was expected; the air in the upper phial was scarcely distinguishable from what it was at first; that in the under phial was still so much nitrous as to require its own bulk of common air to saturate it.

2. The above experiment was repeated, and the upper phial drawn off when the whole was under water, in order to prevent communication with the atmosphere: about $\frac{1}{6}$ of an ounce of water entered the phials, to compensate the diminution. Remaining air in the upper phial was a very little worse than common air, it being of the standard 1,47 when the former was 1,44. The gas in the under phial was still nitrous and nearly of the same purity as at first; for three parts of it required four of atmospheric air to saturate them.

3. Nitrous gas and one $\frac{2}{3}$ oxygenous were tried in the same way: after four hours, the apparatus was taken down under water. The upper phial was $\frac{2}{7}$ filled with water, and the gas

gas in it was partly driven down the tube into the other phial, by which, and the previous procefs, the nitrous gas was completely faturated and nothing but azotic with a fmall portion of oxigenous were found in the under phial: the remaining gas in the upper phial was ftill $\frac{1}{3}$ oxygenous.

4. Nitrous gas and hydrogenous: in three hours the upper phial was $\frac{1}{4}$ nitrous, and of courfe the under muft have a like part of hydrogen.

5. Nitrous gas and azotic: after three hours the upper phial was $\frac{1}{3}$ nitrous.

In the two laft experiments, the quantity of nitrous gas in the upper phial was lefs than might be expected; but the tube was at firft filled with common air, and fome muft enter on connecting the apparatus, which is fufficient to account for the refults.

FOURTH CLASS.

Azotic Gas, with Mixtures containing Oxigenous Gas.

Azote with oxygenous compounds.

1. Azotic gas and one $\frac{2}{3}$ oxygenous: after ftanding three hours the upper phial was of the ftandard 1.78, or about $\frac{1}{14}$ oxygenous.

2. Azotic gas with atmofpheric air: after ftanding three hours: the upper phial was not fenfibly diminifhed by nitrous gas; the under phial, however, had loft two per cent, or $\frac{1}{10}$ of its oxigen. The reafon of this was, that the azotic gas in this experiment having been juft made for it from nitrous gas, this laft had not been completely faturated with atmofpheric air, and hence had feized upon all the oxygen afcending into the upper phial.

Having now related all the experiments I made of any importance to the fubject, it will be proper to add, for the fake of thofe that may wifh to repeat fome of them, that great care muft be taken to keep the infide of the tube dry; for if a drop of water interpofe between the two gafes, I have found that it effectually prevents the intercourfe: glafs tubes fhould therefore be ufed, that one may be fatisfied on this head, as the obftruction will then be vifible.

I fhall make no further comments on the above experiments, by way of explanation: becaufe to thofe who underftand my hypothefis of elaftic fluids, they need none: and I think it would be in vain to attempt an explanation any other way.

I can

I cannot however, on this occasion, avoid adverting to some experiments of Dr. Priestley, which few modern philosophers can be unacquainted with: I mean those relating to the seeming conversion of water into air. (Vid. Philos. Transact. vol. 73, page 414,—or his Expts. abridged, vol. 2, page 407.) He found that unglazed earthern retorts containing a little moisture, when heated, admitted the external air to pass through their pores at the same time that aqueous vapour passed through the pores the contrary way or outward; and that this last circumstance was *necessary* to the air's entrance. The retorts are air-tight, so far as that blowing into them discovers no pores; but when subjected to a greater pressure, as that of the atmosphere, or even one much short of it, they are not able to prevent the passage of elastic fluids. The fact of air passing into the retort through its pores, and vapour out of them at the same time, are elegantly and most convincingly shewn by Dr. Priestley's experiments, in which he used the apparatus represented in plate 7, fig. 1, of the edition above referred to. The Doctor confesses his explanation of these remarkable facts is very inadequate; and no wonder, for it is impossible for him or any other to explain them on the commonly received principles of elastic fluids. But we will hear what he says on the subject:—"At present it is my opinion, that the agent in this case is that principle which we call *attraction* of *cohesion*, or that power by which water is raised in capillary tubes. But in what manner it acts in this case I am far from being able to explain. Much less can I imagine how *air* should pass one way and *vapour* the other, in the same pores, and how the transmission of the one should be necessary to the transmission of the other.—I am satisfied, however, that it is by means of such pores as air may be forced through, that this curious process is performed; because the experiment never succeeds but in such vessels as, by the air-pump at least, appear to be porous, though in all such."

The remarkable experiment of Priestley, of air entering earthen retorts while water passed out in a vapor.

Dr. Priestley's explanation or conjectures.

The truth is, these facts so difficult to explain are exactly similar to those which are the subject of this memoir: only instead of a *great number* of pores we have *one* of sensible magnitude, (the bore of the tube.) Let the porous retort have the same elastic fluid within and without, in the one case; and the two phials contain the same elastic fluid in the other, then

The fact is, that the steam and air mix by means of the pores.

then no tranfmiffion is obfervable in either; but if the retort have common air, or any other gas, without, and aqueous vapour, or any other elaftic fluid, except the outfide one, within; then the motion in and out commences, juft as with the phials in fimilar circumftances. In fact this laft obfervation has fince been verified by Dr. Prieftley himfelf, of which an account is given in No. 2, of the American Philofophical Tranfactions, vol. 5. After alluding to his experiments abovementioned, he obferves, "Since that time I have extended and diverfified the experiments, and have obferved, that what was done by *air* and *water*, will be done by *any two kinds of air*, and whether they have affinity to one another or not, that this takes place in circumftances of which I was not at all apprized before, and fuch as experimenters ought to be acquainted with, in order to prevent miftakes of confiderable confequence."

—and the fame happens in any two gafes.

The facts ftated above, taken altogether, appear to me to form as decifive evidence *for* that of elaftic fluids which I maintain, and *againft* the one commonly received, as any phyfical principle which has ever been deemed a fubject of difpute, can adduce.

III.

On the Horizontal Moon. By Dr. OKELY. *In a Letter from* *Mr.* H. STEINHAUER.

To Mr. NICHOLSON.

SIR, *Fulnuk, March* 1, 1806.

ABOUT the beginning of laft year, I had the pleafure, in compliance with your obliging letter to fend you impreffions of the Egyptian Scarabacus, which I hope came fafe to hand. Your kindnefs in inferting my trivial remarks upon the fame in your valuable Journal, encourage me to fubmit the following fhort effay, upon a fubject which has employed the ingenuity of feveral of your correfpondents, which I received from my friend Dr. Okely, of Wyke, near Hallifax, in confequence of fome converfations occafioned by the perufal of your work.

If

If you think it worthy a place in your collection, it will be confidered as an additional obligation conferred on,

SIR,

Your obedient fervant,

H. STEINHAUER.

Obfervations on the feemingly enlarged apparent Diameters of the Sun and Moon, when viewed in or near the Horizon.

Every one who views the fun or moon, when they are in the horizon, thinks that they appear larger than when they are feen in any more elevated part of the heavens. And aftronomers know, that the diftance of the fame fixed-ftars is apparently greater when feen near the horizon, than when they are more elevated. But it is likewife well known to aftronomers, that the apparent magnitudes of the fun and moon, as well as the apparent diftances of any given fixed ftars, as meafured by the micrometer, are the fame in that part of the heavens which is near the horizon, as, in the fame circumftances, they are found to be in any other part, except that the moon, being really perceptibly farther from an obferver, placed on the earth's furface, when fhe appears in the horizon, than when fhe appears in the zenith, is found to have a fmaller apparent magnitude, agreeing with the caufes to which it is known to be owing. The firft mentioned phenomena muft therefore belong to the head of *optical deceptions*. Let us enquire from what fource this deception arifes.

General fact ftated that the heavenly bodies feem larger at low altitudes.

I am not the firft by whom the fource was fought for in the apparent flatnefs of the fky; but I differ as far as I know, from all others in my manner of connecting one appearance with the other.

In order to explain my idea of the matter, I fhall firft attempt to fhew that the flattened appearance of the vifible heavens is not an illufion, but a reality; or in other words, that an obferver placed on the earth is really at a greater diftance from a point of the fky, fituate in the horizon, than from a point fituate in the zenith.

Explanation of the flattened appearance of the heavens.

This will appear in the cleareft manner if we endeavour to give an anfwer to the two following queftions: *What is the fky?* and *Where is the fky?*

The fky is a real object, and offers a flattened concavity.

By the fky, I mean that blue concave fuperficies, within which every obferver on the furface of the earth finds himfelf

placed

placed. What is this? It is certainly something real and material, or else it would not appear coloured. For bodies, to appear coloured, must have parts of some determinate magnitude.

Where doth it exist? Not in those immensely distant parts of space, where the heavenly bodies revolve. For if those spaces contained any bodies of a determinate magnitude, and consequently of a determinate density, the heavenly bodies could not continue through ages to revolve in the same periodic times; their momentum would be diminished by resistance, and the periodic times of their revolution would change. The blue sky therefore cannot be placed beyond the atmosphere of our earth. The smallest parts of bodies, that are coloured are blue, and the blue sky is therefore either the atmosphere itself or the smallest and most elevated vapours ascending in it, or both together. The heavenly bodies shine through it, and therefore it cannot be opaque; it is itself of a blue colour, and therefore is not perfectly transparent.

Though we are ignorant of the exact height of the atmosphere, yet we may take it for granted, that it does not extend as far as the moon, and therefore that the distance of its farthest points from the centre of the earth has a finite ratio to the semi-diameter of the earth. That the ratio is probably less than 2 : 1.

If therefore the blue heavens which surround the earth, and are concentric with it, have a semidiameter not double that of the earth, their horizontal points as viewed from the earth, must be farther from us than any that are nearer the zenith.

For let A C F (*Plate* VII. *Fig.* 1.) represent a great circle of the earth, and A C be its radius, and let the circle D B E G represent a great circle of the atmosphere drawn with a radius A B not $= 2 . AC$, the line $CD > BC$; $HC > BC$.

—which is not always alike.

I was led to this solution of the flattened appearance of the heavens, by observing that, when the sky is uniformly overcast with clouds, the concave superficies appears considerably flatter than when the sky is serene. In the former case, the two concentric circles in the figure approach nearer to each other, the clouds being nearer to the earth than the sky is, and the ratio of D E to C B must of course increase.

Explanation from a diagram of the sky, that

But to proceed. When any bodies situated behind a semitransparent screen are seen through it, they will appear to be fixed

fixed in the fcreen at the points of interfection, which lines drawn from every point of the bodies to the eye of the obferver, make with the fcreen. Now fuch a femi-tranfparent fcreen, the blue fkies interpofe between the heavenly bodies and our eyes. They will therefore appear to be fixed in the fky, at the above-mentioned points of interfection. the heavenly bodies muft appear largeft in the horizon.

But if lines D C, H C, I C, B C, be drawn fo that the angle at C are equal, they may be confidered as coming from the extreme points of bodies which fubtend equal angles of vifion, or which have the fame apparent magnitude. The angle D C H may be confidered as reprefenting the angle of vifion which the fun fubtends at the horizon. The equal angle H C I, the angle fubtended by the fame body in a more elevated fituation. But D H $>$ H I. Thus the heavenly bodies muft appear enlarged in their vertical diameters, when in the horizon; and the fame may be fhewn of any other diameter They will therefore appear uniformly enlarged; which was the thing to be explained.

W. OKELY.

IV.

Account of fome Specimens of Bafaltes from the northern Coaft of Antrim. By the Rev. Dr. WILLIAM RICHARDSON.

(Concluded from Page 273.)

THE peninfula of Portrufh lies about fix miles to the weft of the Giant's Caufeway, and on its eaftern furface alone prefents thefe ftrata. Remark on the bafaltes of the coaft of Antrim.

In the fpace of about 700 yards, it exhibits in miniature thofe changes and interruptions of the ftrata, which occur on the large fcale along the northern bafaltic coaft of Ireland. At the place where it emerges from the ftrand, there firft occurs a mafs compofed of ftrata of the coarfe and filiceous bafalt, placed over each other alternately; this is fucceeded by an accumulation of regular ftrata of the coarfe bafalt alone. A fecond alternation, and a fecond accumulation of the coarfe-grained ftrata, come in order, and extend to the well called Tubber Wherry. Here commences an accumulation of many ftrata of the fili-

ceous

Remarks on the basaltes of the coast of Antrim.

ceous basalt alone, which stretches along the shore for about 100 yards, and then changes into a third alternation, which continues to the little boat-harbour, called *Port-in-too*, near which the siliceous basalt disappears. Over this stretch, notwithstanding the frequent change in the arrangement of the strata, the thickness of each stratum, of both species, remains pretty nearly the same, and the position of them all steadily so, viz. with a considerable dip to E. N. E.

The west side of the peninsula, though only about 400 yards distant, consists entirely of coarse basalt. It shows a bolder face, and is formed of rude massive pillars, from 60 to 80 feet long.

"I am aware," says Dr. Richardson, "that several mineralogists deny the shell-bearing stone to be basalt, while others contend strenuously that it is. I will not venture to decide on the question, but must remark, that I have never met with it but contiguous to basalt, and so solidly united to this last, that the continuity of the whole mass was uninterrupted. The grain of the stone graduates, as has been already remarked, into that of the common basaltes; and the arrangement of it and that of the basalt, with which it is so much mixed at Portrush and the Skerry island, is exactly the same; the strata of each scarcely differing in thickness, and not at all in inclination. The strata of both kinds break into prisms, and the surfaces, where accessible, exhibit the appearance of causeways, differing only in this, that in the siliceous basalt, the pentagon is the prevalent figure, and in the coarse basalt, the quadrangle. The fusibility of both stones is also nearly the same; the shells in the siliceous basalt are calcined in the fire, and many more are then discovered which had before escaped the eye *."

Whinstone

* Dr. Richardson observes, that some mineralogists deny that this fossil is basalt. Several of the members present when this paper was read, some of whom had examined the stone in its native place, were of that number. It was remarked, that though certain portions of the strata of this fossil bore much resemblance to some species of basalt, by far the greater part of the mass bore no resemblance whatever to any.

It was also stated, that the substance of the coarse-grained, undisputed basalt, which lies between the strata of this stone, does not contain any vestiges of marine animals; That veins often issue from

Whinstone Dikes on the Coast of Antrim.

Remarks on the basaltes of the coast of Antrim.

Dr. Richardson describes some particulars in the construction of the whinstone dikes on the coast of Antrim, which appear singular, and deserving of attention. These dikes, he says, are uniformly formed of large massive prisms laid horizontally, which are always divisible into smaller prisms that are likewise horizontal. To prevent confusion, he calls the first of these *component prisms*, and the second, or smaller ones into which the others break, *constituent prisms*.

The component prisms are sometimes of enormous size, and in the same dike are nearly equal; the constituent prisms are small, (the sides about an inch long), and neatly formed.

The dike which traverses the Giant's Causeway, differs from those on other parts of the coast, by having no component prisms. It resembles a plain wall, of which the parts shiver under the hammer into very neat constituent prisms, In the dike at Seaport the same thing is observed; the prismatic structure does not penetrate two inches from its edge; the whole interior seems an amorphous mass.

The specimens of this latter dike, sent to Dr. Hope, exhibit its continuity with the adjacent basaltic rock which it traverses, and also the continuity of the fine basalt of its edge with the *granular* stone which composes the middle of the dike.

The dike of *Port-coan* is a very solid mass, composed of stones apparently round, and imbedded in a basaltic paste, or indurated mortar. The round stones are formed of concentric spheres, like the coats of an onion; they exceed a foot in diameter, and, together with the mortar by which they are united, they form a very compact and highly indurated rock.

Besides these large dikes, Dr. Richardson remarks, that veins from half an inch to an inch and a half thick, often cut the basaltic strata on that coast in all directions. The materials of these veins are never the same with the contiguous basalt,

from the beds of this real basalt, and pervade the supposed siliceous species; some of them connecting together the separate beds of the real basalt; others dying away in slender ramifications; as they rise through the interposed stratum. In no instance is this reversed: The veins never proceed from what is called the Siliceous Basalt. It was farther observed, that both the fracture and external surface of this stone exhibit a stratified structure, in many Instances, which never happens in the true basaltes.

but

Remarks on the basaltes of the coast of Antrim.

but are generally finer. At Portrush is a large vein, and near it a smaller vein, not an inch thick, which, proceeding from below, terminates in the solid rock before it reaches the surface.

Miscellaneous Observations.

Some of the specimens in Dr. Richardson's catalogue are from a quarry in a mass of basalt at Ballylugan, two miles south of Portrush. This basalt contains small cavities in its interior; many of them full of fresh water, which gushes out when the stone is broken by the hammer, as if it had been in a state of compression. The stone is so hard, and flies so in pieces, that Dr. Richardson has not been able to collect any of the water for the purpose of analysis.

The face of the quarry in which this variety of the basalt is found is about 15 feet high, and is cut into a stratum, the thickness of which is not yet ascertained. The rock is entirely columnar, the pillars somewhat smaller than those of the Giant's Causeway, less perfect, not articulated, sometimes bent, and variously inclined. The sides and the interior of the pillars are full of cavities. In consequence of the observations of Dr. Hamilton and Mr. Whitehurst respecting the porous texture of the air or bladder holes of the basaltes of the Causeway and its vicinity, Dr. Richardson has examined a great variety; but in no instance, except this of Ballylugan, has he found cavities, in the interior of the basaltic rocks on this coast, though they are frequent on the surface exposed to the air.

The last variety of whinstone enumerated by Dr. Richardson is the Ochrous, which makes, as he says, a conspicuous figure in the stupenduous precipices along the coast of Antrim. It is disposed in extensive strata of every thickness, from an inch to twenty-four feet, and varies in colour, from a bright minium to a dull ferruginous brown.

Three remarks are made by Dr. Richardson, that are undoubtedly of importance, and show that this stone is merely basalt in a certain state of decomposition.

1. The ochrous strata are extensive; they remain always parallel to the basalt strata which they separate; they unite to the basalt without interrupting its solidity; the change from the

one to the other is fudden, and the lines of demarkation are diftinct. The ochrous ftone is never found but contiguous to other bafalt.

2. The fubftances imbedded in the ochrous rock, and in bafalts, are exactly the fame; calcareous fpar, zeolite, chalcedony, &c.

3. Among the varieties which this rock prefents, there may be found every intermediate ftage between found bafalt and perfect ochre. The change is often partial, beginning with veins and flender ramifications.

V.

On the Abforption of Gafes by Water and other Liquids. By John Dalton.*

1. If a quantity of pure water be boiled rapidly for a fhort time in a veffel with a narrow aperture, or if it be fubjected to the air-pump, the air exhaufted from the receiver containing the water, and then be brifkly agitated for fome time, very nearly the whole of any gas the water may contain, will be extricated from it. Air or gas is extricated from water by boiling and agitation in vacuo.

2. If a quantity of water thus freed from air be agitated in any kind of gas, not chemically uniting with water, it will abforb its bulk of the gas, or otherwife a part of it equal to fome one of the following fractions, namely, $\frac{1}{8}$, $\frac{1}{27}$, $\frac{1}{64}$, $\frac{1}{125}$, &c. thefe being the cubes of the reciprocals of the natural numbers 1, 2, 3, &c. or $\frac{1}{1^3}$, $\frac{1}{2^3}$, $\frac{1}{3^3}$, $\frac{1}{4^3}$, &c. the fame gas always being abforbed in the fame proportion, as exibited in the following table:—It muft be underftood that the quantity of gas is to be meafured *at the preffure and temperature with which the impregnation is effected.* The volume of every gas abforbed by water is conftant, and is either equal to the bulk or to the cube of a reciprocal of that bulk, —equal preffures and temperatures being fuppofed.

* Manchester Mem. N. S. Vol. I.

Bulk

Table of quantities.

Bulk abforbed, the bulk of water being unity. $\frac{1}{1^3} = 1$	Carbonic acid gas, fulphuretted hydrogen, nitrous oxide.*
$\frac{1}{2^3} = \frac{1}{8}$	Olefiant gas, of the Dutch chemifts.
$\frac{1}{3^3} = \frac{1}{27}$	Oxygenous gas, nitrous gas,† carburretted hydrogen gas, from ftagnant water.
$\frac{1}{4^3} = \frac{1}{64}$	Azotic gas, hydrogenous gas, carbonic oxide.
$\frac{1}{5^3} = \frac{1}{125}$	None difcovered.

3. The gas thus abforbed may be recovered from the water the fame in quantity and quality as it entered, by the means pointed out in the firft article.

Water abforbs any gas in the fame quantity, whether it contain another gas or not.

4. If a quantity of water free from air be agitated with a mixture of two or more gafes (fuch as atmofpheaic air) the water will abforb portions of each gas the fame as if they were prefented to it feparately in their proper denfity.

Ex. gr. Atmofpheric air, confifting of 79 parts azotic gas, and 21 parts oxygenous gas, *per cent.*

Water abforbs $\frac{1}{64}$ of $\frac{79}{100}$, azotic gas = 1.234
———— $\frac{1}{27}$ of $\frac{21}{100}$, oxygen gas = .778

Sum, *per cent.* 2.012

* According to Mr. William Henry's experiments, water does not imbibe quite its bulk of nitrous oxide; in one or two inftances with me it has come very near it: The apparent deviation of this gas, may be owing to the difficulty of afcertaining the exact degree of its impurity.

† About $\frac{1}{20}$ of nitrous gas is ufually abforbed; and $\frac{1}{27}$ is recoverable: This difference is owing to the refiduum of oxygen in the water, each meafure of which takes $3\frac{1}{2}$ of nitrous gas to faturate it, when in water. Perhaps it may be found that nitrous gas ufually contains a fmall portion of nitrous oxide.

5. If

If water and gases be agitated in confinement, a mixture will take place of the gases in and out of the water, &c.

5. If water impregnated with any one gas (as hydrogenous) be agitated with another gas *equally* absorbable (as azotic) there will *apparently* be no absorption of the latter gas; just as much gas being found after agitation as was introduced to the water; but upon examination the residuary gas will be found a *mixture* of the two, and the parts of each, in the water, will be exactly proportional to those out of the water.

6. If water impregnated with any one gas be agitated with another gas less or more absorbable; there will *apparently* be an increase or diminution of the latter; but upon examination the residuary gas will be found a *mixture* of the two, and the proportions agreeable to article 4.

Temperature does not affect such inclosed fluids.

7. If a quantity of water in a phial having a ground stopper very accurately adapted, be agitated with any gas, or mixture of gases, till the due share has entered the water; then, if the stopper be secured, the phial may be exposed to any variation of *temperature*, without disturbing the equilibrium: That is, the quantity of gas in the water will remain the same whether it be exposed to heat or cold, if the stopper be air-tight.

N. B. The phial ought not to be near full of water, and the temperature should be between 32° and 212°.

Gases which are disposed to combine.

8. If water be impregnated with one gas (as oxygenous), and another gas, having an affinity for the former (as nitrous), be agitated along with it; the absorption of the latter gas will be greater, by the quantity necessary to saturate the former, than it would have been if the water had been free from gas.*

The absorption by other liquids is the same as by water.

9. Most liquids free from viscidity, such as acids, alcohol, liquid sulphurets, and saline solutions in water, absorb the same quantity of gases as pure water; except they have an affinity for the gas, such as sulphurets for oxygen, &c.

The preceding articles contain the principal facts necessary to establish the theory of absorption: Those that follow are of a subordinate nature, and partly deducible as corollaries to them.

* One part of oxygenous gas requires 3.4 of nitrous gas to saturate it in water. It is agreeable to this that the rapid mixture of oxygenous and nitrous gas over a broad surface of water, occasions a greater diminution than otherwise. In fact, the *nitrous* acid is formed this way; whereas, when water is not present, the *nitric* acid is formed, which requires just half the quantity of nitrous gas, as I have lately ascertained.

Natural waters or rain contain the due share of atmos. air; but corrupt water has less or no oxygen.

10. Pure distilled water, rain and spring water usually contain nearly their due share of atmospheric air: if not, they quickly acquire that share by agitation in it, and lose any other gas they may be impregnated with. It is remarkable, however, that water by stagnation, in certain circumstances, loses part or all of its oxygen, notwithstanding its constant exposition to the atmosphere. This I have uniformly found to be the case in my large wooden pneumatic trough, containing about eight gallons, or $1\frac{1}{3}$ cubic foot of water. Whenever this is replenished with tolerably pure rain water, it contains its share of atmospheric air; but in process of time it becomes deficient of oxygen: In three months the whole surface has been covered with a pellicle, and no oxygenous gas whatever was found in the water. It was grown offensive, but not extremely so; it had not been contaminated with any material portion of metallic or sulphureous mixtures, or any other article to which the effect could be ascribed.* The quantity of azotic gas is not materially diminished by stagnation, if at all.—These circumstances, not being duly noticed, have been the source of great diversity in the results of different philosophers upon the quantity and quality of atmospheric air in water. By article 4, it appears that atmospheric air expelled from water ought to have 38 per cent. oxygen; whereas by this article air may be expelled from water that shall contain from 38 to 0 per cent. of oxygen. The disappearance of oxygenous gas in water, I presume, must be owing to some impurities in the water which combine with the oxygen. Pure rain water that had stood more than a year in an earthenware bottle had lost none of its oxygen.

Why water by agitation absorbs most oxygen from air.

11. If water free from air be agitated with a small portion of atmospheric air (as $\frac{1}{15}$ of its bulk) the residuum of such air will have proportionally less oxygen that the original: If we take $\frac{1}{15}$, as above, then the residuum will have only 17 per cent. oxygen; agreeably to the principle established in article 4. This circumstance accounts for the observations made by Dr. Priestley, and Mr. William Henry, that water absorbs oxygen in preference to azote.

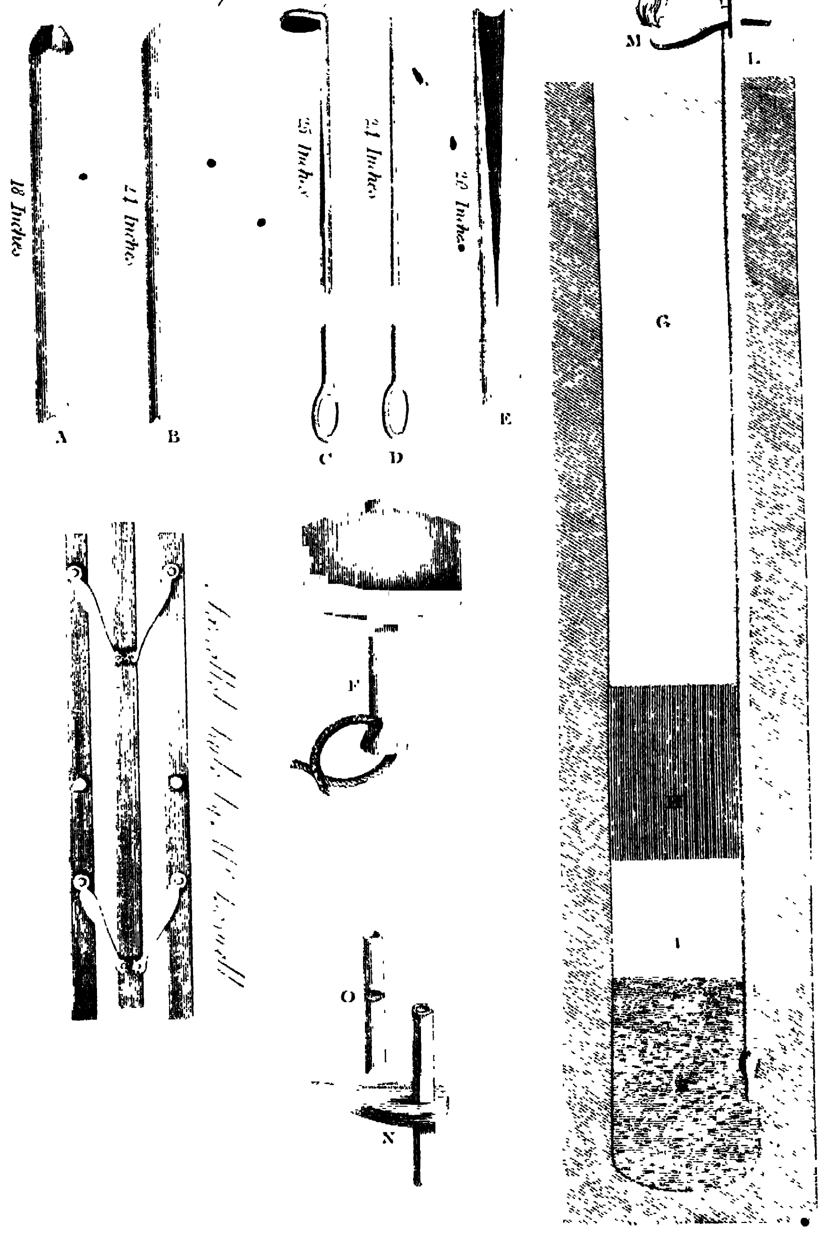
Disappearance of gas by agitation under a jar.

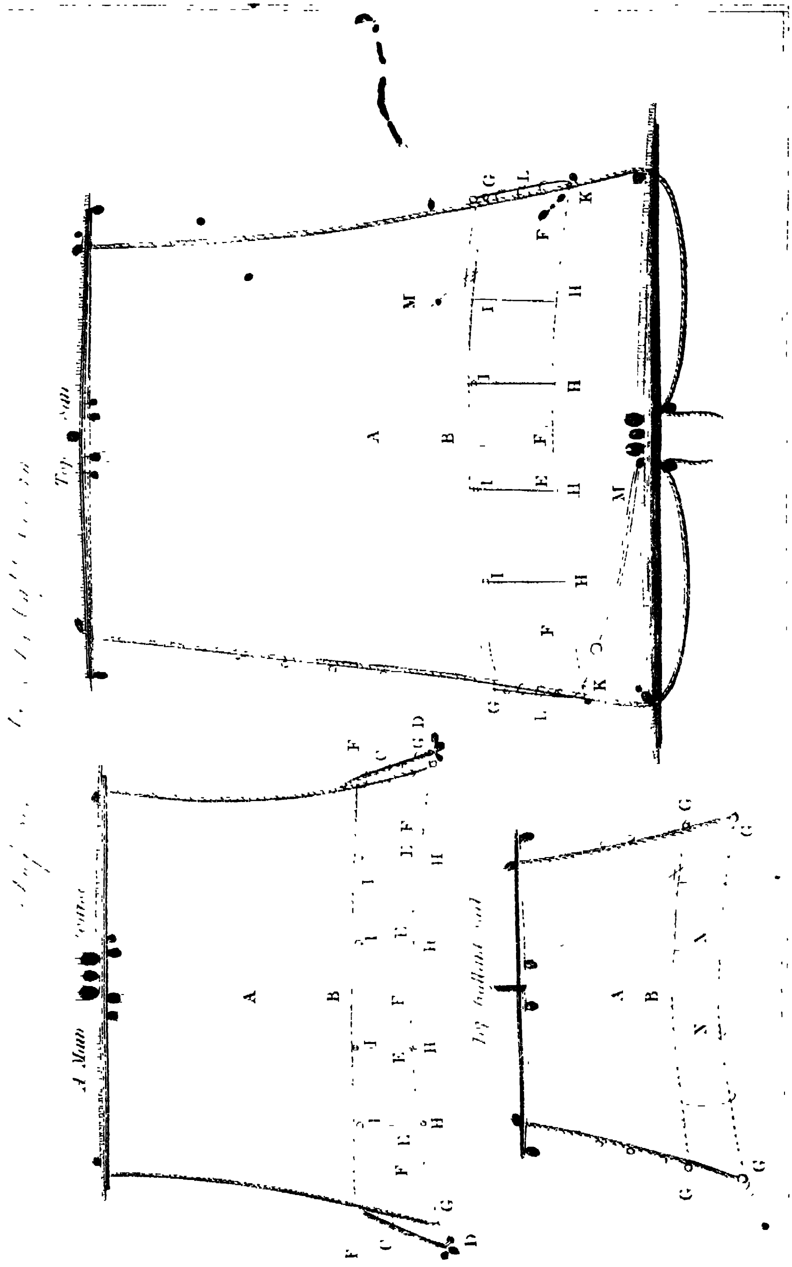
12. If a tall glass vessel, containing a small portion of gas be inverted into a deep trough of water, and the gas thus confined by the glass and the water be briskly agitated, it will gradually disappear.

* It was drawn from a leaden cistern.

It

Method of blasting Rocks.




Top Sail
A Main
Top gallant sail
A
B
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It is a wonder that Dr. Priestley, who seems to have been the first to notice this fact, should have made any difficulty of it;—the loss of gas has evidently a mechanical cause; the agitation divides the air into an infinite number of minute bubbles, which may be seen pervading the whole water; these are successively driven out from under the margin of the glass into the trough, and so escape.

13. If old stagnant water be in the trough, in the last experiment, and atmospheric air be the subject, the oxygenous gas will very soon be almost wholly extracted, and leave a residuum of azotic gas; but if the water be fully impregnated with atmospheric air at the beginning, the residuary gas examined at any time will be pure atmospheric air. Old stagnant water.

14. If any gas not containing either azotic or oxygenous gas, be agitated over water containing atmospheric air, the residuum will be found to contain both azotic and oxygenous gas. Agitation of gas over common water gives out oxygen and azote by mixture.

15. Let a quantity of water contain equal portions of any two or more unequally absorbable gases: For instance, azotic gas, oxygenous gas, and carbonic acid gas; then, let the water be boiled or subjected to the air-pump, and it will be found that unequal portions of the gases will be expelled. The azotic will be the greatest part, the oxygenous next, and the carbonic acid will be the least. For, the previous impregnation being such as is due to atmospheres of the following relative forces nearly: The escape of any gas from water by removing the pressure will be greater the less absorbable.

Azotic	- - -	21 inch. of mercury.
Oxygenous	- -	9
Carbonic acid	-	$\frac{1}{3}$

consequently, when those forces are removed, the resiliency of the azotic gas will be the greatest, and that of the carbonic acid the least; the last will even be so small as not to overcome the cohesion of the water without violent agitation.

Remarks on the Authority of the preceding Facts.

In order to give the chain of facts as distinct as possible, I have not hitherto mentioned by whom or in what manner they were ascertained. Remarks on the laws of absorption of gases by dense fluids, &c.

The fact mentioned in the first article has been long known; a doubt, however, remained respecting the quantity of air still

 left

Remarks on the laws of absorption of gases by dense fluids, &c.

left in water after ebullition and the operation of the air-pump. The subsequent articles will, I apprehend, have placed this in a clearer point of view.

In determining the quantity of gases absorbed, I had the result of Mr. William Henry's experience on the subject before me, an account of which has been published in the Philosophical Transactions for 1803. By the reciprocal communications since, we have been enabled to bring the results of our experiments to a near agreement; as the quantities he has given in his appendix to that paper nearly accord with those I have stated in the second article. In my experiments with the less absorbable gases, or those of the 2d, 3d, and 4th classes, I used a phial holding 2700 grains of water, having a very accurately ground stopper; in those with the more absorbable of the first class, I used an eudiometer tube, properly graduated, and of aperture so as to be covered with the end of a finger. This was filled with the gas and a small portion expelled by introducing a solid body under water; the quantity being noticed by the quantity of water that entered on withdrawing the solid body, the finger was applied to the end and the water within agitated; then removing the finger for a moment under water, an additional quantity of water entered, and the agitation was repeated till no more water would enter, when the quantity and quality of the residuary gas was examined. In fact, water could never be made to take its bulk of any gas by this procedure; but if it took $\frac{9}{10}$, or any other part, and the residuary gas was $\frac{9}{10}$ pure, then it was inferred that water would take its bulk of that gas. The principle was the same in using the phial; only a small quantity of the gas was admitted, and the agitation was longer.

There are two very important facts contained in the second article. The first is, that the quantity of gas absorbed is as the density or pressure. This was discovered by Mr. William Henry, before either he or I had formed any theory on the subject.

The other is, that the density of the gas in the water has a special relation to that out of the water, the distance of the particles within being always some multiple of that without: Thus, in the case of carbonic acid, &c. the distance within and without is the same, or the gas within the water is of the same density as without; in olefiant gas the distance of the

particles

particles in the water is twice that without; in oxygenous gas, &c. the distance is just three times as great within as without; and in azotic, &c. it is four times. This fact was the result of my own enquiry. The former of these, I think, decides the effect to be mechanical; and the latter seems to point to the principle on which the equilibrium is adjusted.

Remarks on the laws of absorption of gases by dense fluids, &c.

The facts noticed in the 4th, 5th and 6th articles, were investigated *à priori* from the mechanical hypothesis, and the notion of the distinct agency of elastic fluids when mixed together. The results were found entirely to agree with both, or as nearly as could be expected from experiments of such nature.

The facts mentioned in the 7th article are of great importance in a theoretic view; for, if the quantity of gas absorbed depend upon mechanical principles, it cannot be affected by temperature in confined air, as the mechanical effect of the external and internal air are alike increased by heat, and the density not at all affected in these circumstances. I have tried the experiments in a considerable variety of temperature without perceiving any deviation from the principle. It deserves further attention.

If water be, as pointed out by this essay, a mere receptacle of gases, it cannot affect their affinities: hence what is observed in the 8th article is too obvious to need explanation.— And if we find the absorption of gases to arise not from a chemical but a mechanical cause, it may be expected that all liquids having an equal fluidity with water, will absorb like portions of gas. In several liquids I have tried, no perceptible difference has been found; but this deserves further investigation.

After what has been observed, it seems unnecessary to add any explanation of the 10th and following articles.

Theory of the Absorption of Gases by Water, &c.

From the facts developed in the preceding articles, the following theory of the absorption of gases by water seems deducible.

1. All gases that enter into water and other liquids by means of pressure, and are wholly disengaged again by the removal of that pressure, are *mechanically* mixed with the liquid, and not *chemically* combined with it.

2. Gases

Remarks on the laws of absorption of gases by dense fluids, &c.

2. Gases so mixed with water, &c. retain their elasticity or repulsive power amongst their own particles, just the same in the water as out of it, the intervening water having no other influence in this respect than a mere vacuum.

3. Each gas is retained in water by the pressure of gas of its own kind incumbent on its surface abstractedly considered, no other gas with which it may be mixed having any permanent influence in this respect.

4. When water has absorbed its bulk of carbonic acid gas, &c. the gas does not press on the water at all, but presses on the containing vessel just as if no water were in. When water has absorbed its proper quantity of oxygenous gas, &c. that is, $\frac{1}{27}$ of its bulk, the exterior gas presses on the surface of the water with $\frac{26}{27}$ of its force, and on the internal gas with $\frac{1}{27}$ of its force, which force presses upon the containing vessel, and not on the water. With azotic and hydrogenous gas the proportions are $\frac{63}{64}$ and $\frac{1}{64}$ respectively. When water contains no gas, its surface must support the whole pressure of any gas admitted to it, till the gas has, in part, forced its way into the water.

5. A particle of gas pressing on the surface of water is analogous to a single shot pressing upon the summit of a square pile of them. As the shot distributes its pressure equally amongst all the individuals forming the lowest stratum of the pile, so the particle of gas distributes its pressure equally amongst every successive horizontal stratum of particles of water downwards till it reaches the sphere of influence of another particle of gas. For instance; let any gas press with a given force on the surface of water, and let the distance of the particles of gas from each other be to those of water as 10 to 1; then each particle of gas must divide its force equally amongst 100 particles of water, as follows:—It exerts its immediate force upon 4 particles of water; those 4 press upon 9, the 9 upon 16, and so on according to the order of square numbers, till 100 particles of water have the force distributed amongst them; and in the same stratum each square of 100, having its incumbent particle of gas, the water below this stratum is uniformly pressed by the gas, and consequently has not its equilibrium disturbed by that pressure.

6. When water has absorbed $\frac{1}{27}$ of its bulk of any gas, the stratum of gas on the surface of the water presses with $\frac{26}{27}$ of

its force on the water, in the manner pointed out in the last article, and with $\frac{1}{27}$ of its force on the uppermost stratum of gas in the water: The distance of the two strata of gas must be nearly 27 times the distance of the particles in the incumbent atmosphere, and 9 times the distance of the particles in the water. This comparatively great distance of the inner and outer atmosphere arises from the great repulsive power of the latter, on account of its superior density, or its presenting 9 particles of surface to the other 1. When $\frac{1}{64}$ is absorbed, the distance of the atmospheres becomes 64 times the distance of two particles in the outer, or 16 times that of the inner. The annexed views of perpendicular and horizontal strata of gas in and out of water, will sufficiently illustrate these positions.

Remarks on the laws of absorption of gases by dense fluids, &c.

7. An equilibrium between the outer and inner atmospheres can be established in no other circumstance than that of the distance of the particles of one atmosphere being the same or some multiple of that of the other; and it is probable the multiple cannot be more than 4. For in this case the distance of the inner and outer atmospheres is such as to make the perpendicular force of each particle of the former on those particles of the latter that are immediately subject to its influence, physically speaking, equal; and the same may be observed of the small lateral force.

8. The greatest difficulty attending the mechanical hypothesis, arises from different gases observing different laws.— Why does water not admit its bulk of every kind of gas alike? This question I have duly considered, and though I am not yet able to satisfy myself completely, I am nearly persuaded that the circumstance depends upon the weight and number of the ultimate particles of the several gases: those whose particles are lightest and single being least absorbable, and the others more, according as they increase in weight and complexity.* An enquiry into the relative weights of the ultimate particles of bodies, is a subject, as far as I know, entirely new: I have lately been prosecuting this enquiry with remarkable success. The principle cannot be entered upon in this paper; but I shall just subjoin the results, as far as they appear to be ascertained by my experiments.

* Subsequent experience renders this conjecture less probable.

Table

Weights of the particles of bodies.

Table of the relative weights of the ultimate particles of gaseous and other bodies.

Hydrogen	1
Azote	4.2
Carbon	4.3
Ammonia	5.2
Oxygen	5.5
Water	6.5
Phosphorus	7.2
Phosphuretted hydrogen	8.2
Nitrous gas	9.3
Ether	9.6
Gaseous oxide of carbon	9.8
Nitrous oxide	13.7
Sulphur	14.4
Nitric acid	15.2
Sulphuretted hydrogen	15.4
Carbonic acid	15.3
Alcohol	15.1
Sulphureous acid	19.9
Sulphuric acid	25.4
Carburetted hydrogen from stagnated water	6.3
Olefiant gas	5.3

VI.

On the supposed fascinating Power of the Rattle-snake. With a remarkable Indian Tradition upon which it is probable the early European Settlers founded their popular Tales. From the Philadelphia Medical and Physical Journal, by BENJAMIN SMITH BARTON, *M. D.*

Fascinating power of the rattle-snake described by Fabricius.

ALMOST all amphibious animals (says Professor Fabricius,) the tortoise excepted, live by preying upon other animals. But being destitute of strength and swiftness, nature has given, at least to some of them (according to the testimony of many and creditable writers,) the peculiar faculty of forcing other animals to throw themselves into their open jaws. Kalm, the Swede, and the American Smith Barton, assert of the

Americans

American ſerpents, that if they fix their fiery, glaring eyes upon any animal, ſuch as a ſquirrel, or a bird, within a certain diſtance, they entirely loſe the power of eſcaping, but throw themſelves, ſlowly, irreſiſtibly, into the extended jaws of the ſnake. And if any thing diſturbs the ſnake, ſo that it withdraws its eyes but for one moment, they eſcape with the utmoſt precipitation.

We obſerve (continues this learned naturaliſt) ſomething ſimilar to this in our common, tardy, thick, and fat toads, which frequently ſit under little ſtones and buſhes, having their mouths wide open, into which flies, bees, and other inſects, are drawn in the ſame manner. All the theories that have hitherto been offered to explain theſe appearances appear to me both unnatural and improbable. Indeed, I cannot but doubt the reality of the fact itſelf, until we ſhall receive further obſervations and diſcoveries relative to it.

J. C. Fabricii, &c.
Reſultate Natur-Hiſtoriſcher Vorleſungen, *p.* 267, 268. *Kiel:* 1804.

It will be evident to any one, who has peruſed, with attention, my two publications * on the ſuppoſed faſcinating faculty of the rattle-ſnake, and other American ſerpents, that Mr. Fabricius has by no means fully comprehended my peculiar theory. I have not adopted the hypotheſis of the very reſpectable Kalm, with whoſe name mine is mentioned by the Daniſh Profeſſor. On the contrary, I have endeavoured to ſhow, and I flatter myſelf that I have very ſatisfactorily ſhown, that there is no ſolid foundation for the vulgar, and very generally-received opinion, that ſerpents are endued with the faculty of faſcinating, or charming, other animals. Annotation by Dr. Barton.

B. S. B.

The following very curious tradition of ſome of our Indians, relative to ſerpents, is worthy of publication in this place. A *part* of the tradition has already been publiſhed in my Narrative.

* A Memoir concerning the Faſcinating Faculty which has been aſcribed to the Rattle-ſnake, and other American Serpents. Philadelphia: 1796.—Supplement to a Memoir, &c. Philadelphia, 1800.—Or ſee Philoſ. Journal, Vols. VII. and VIII.

Supplement

Supplement to a Memoir concerning the Fascinating Faculty which has been ascribed to the Rattle-snake, and other American Serpents.

'Having questioned Indians, a number of times, with respect to snakes having the power of charming, and always being answered in the negative, I was at length desired (says my friend, Mr. John Heckewelder) to give the reason the white people had for believing such a thing, which not being satisfactory, Pemaholend * declared: "The rattle-snake obtains its food merely by slyness, and a persevering patience. It knoweth as well where to watch for its prey as a cat does, and succeeds as well. It has, and retains its hunting grounds. In spring, when the warm weather sets in, and the woods seem alive with the smaller animals, it leaves its den. It will cross a river, and go a mile and further from its den, to the place it intends to spend the summer; and in fall, when all the young animals bred this season are become strong and active, so that they are no more so easily overtaken or caught, it directs its course back again, to its den, the same as a hunter does to his camp.

The rattle-snake catches its prey by craft and address.

"The white-people," continued Pemaholend, "probably have taken the idea of this snake having the power of charming from a tradition of ours (the Indians) which our forefathers have handed down to us, from many hundred years back, and long before ever the white people came into this country. Then (they tell us) there *was* such a snake, and a rattle-snake too, but then there was only *this one* snake which had this power, and he was afterwards destroyed; and since that time it hath never been said that any other of the kind had made its appearance."

Indian tradition.

'At my request, Pemaholend related the tradition, and in the following words. "Our forefathers have told us, that at a small lake, or large pond, not a great distance from where, as is believed, now the great city *Quequenàku* (Philadelphia) is built, there dwelt a rattle-snake, whose length and thickness exceeded that of the thickest and longest tree in the woods. This snake was very destructive, not only in destroying so much game, but in devouring so many Indians: for when he was hungry, he only looked round, and whatever he

American native tradition about a rattle-snake.

* An aged and much respected Delaware-Indian.

saw,

saw, whether Indian, deer, turkey, or even geese flying, he only held his head that way, opening his mouth wide, and drawing breath in the manner we do, and nothing could prevent such living creature entering his jaws. It is even said, that a whole flock of geese, flying at a great distance, have been drawn into his mouth, at one time; * and it was well known among the Indians, that of all the hunters or travellers, who passed that way, very few escaped him.

American native tradition about a rattle-snake.

"The Indians well knew when he was hungry, for then he grew angry, and blew with his mouth, which sounded like thunder: for his breath was so powerful, that all the trees, however large, would bend, and even sometimes break down before him. There being no prospect of ever killing him with arrows, on account of the barrenness of the land far round the lake, into which he would always retire, after satisfying his hunger, a great council of the nation was called together, and the question put, *Where are the Mannittoes of the nation? Are they no more? Shall the whole of the nation be destroyed by a Mannitto-Snake?* At length, two young men, endowed with *Mannittoie powers*, offered their services, and declared, that unless the Mannittoie power of the snake exceeded theirs, they should succeed; but they would, at all events, make an attempt. They then bid farewell to the assembly and their friends, dived into the river, from whence they proceeded under the water to a place opposite the Mennúppeek (lake, or large pond) where this snake dwelt. They made an opening under ground, from the river to the centre of the pond, by which the pond was drained, and became perfectly dry. After returning again, the same way they had come, they found the snake in great uneasiness, and on dry ground. Taking then the advantage of the dry weather, and the grass far around the snake being dry, they set fire to the grass, at a distance, and around the snake, by which means he was burnt

* It is curious, at least, to compare this part of the Indian tradition with what Metrodorus, as cited by Pliny, relates of certain Asiatic serpents. These, he says, by means of their breath, attracted birds, however high they were, or however quick their flight. "Metrodorus, circa Rhyndacum amnem in Ponto, ut supervolantes quamvis alte perniciterque, alites haustu raptas absorbeant." Plin. Hist. Nat. lib. viii. cap. 14.

to

American native tradition about a rattle-snake.

to death.* Thus (continued Pemaholend) was the monster killed by two mannitto men of the nation: for, you must know, in those days, we had such men among us, who could live as well in the water as on land."

'Conversing one day with a Monsy (advanced in years) on ancient times, on the migration of the Indians, &c. he, in order to convince me (says Mr. Heckewelder) what the Indians once were, mentioned the killing of the big snake, the history of which, according to his relation, differing only in the following points:

"a. He did not think it had been a rattle-snake, but understood the old men, from whom he had heard it so often related (when he was young), that it was a snake of a peculiar kind, and had feet; and that never since had a snake of this kind appeared:

"b. That he was not sure as to the place where this snake kept; believed it had been higher up the country, and kept in a wide and deep place of the river, and in the country of the Munsees (or Minsy) and was killed by a Mannitto Munsee:

"c. That after the nation had met in council, and the above questions put, a Munsee man of no character, nor seemingly of any consequence to the nation, said and declared, that he had *Mannittoie Powers*; could and would destroy the monster, prescribing the ceremonies the assembly were to observe during the expedition. That he then made a very strong arrow, or spear, sharp at both ends; and being equipped, took leave of the assembly—plunged into the river, and dived under water, until he arrived within a small distance of the place where the snake lay, or floated, basking in the sun. Here he ascended to the surface, and calling out to the snake to receive him, he opened his mouth wide, and drew him in, when, however, in an instant, the snake was stabbed by him through both his sides, with the spear, which wounded him so deadly, that he gave a whirl, and being under great pain, discharged his excrements, and with the same this hero, who

* Even this part of the Indian tradition seems to be borrowed from the old world. See a curious relation of the capture of an enormous serpent in *The Life of Sethos, as taken from private memoirs of the Egyptians*. Vol. i. p. 125—147. London. 1737.

then,

then ſwam again to ſhore, announcing his victory, and congratulating the aſſembly on the deliverance of the nation.

"Thus (continued the old Munſee) were the Indians of thoſe days *Mannittoes*. Nothing could reſiſt them. They knew nothing of drowning. Our firſt Parents have ſprung from the bottom of a lake."

VII.

A Deſcription of a Property of Caoutchouc, or Indian Rubber; With ſome Reflections on the Cauſe of the Elaſticity of this Subſtance. In a Letter to Dr. HOLME.*

SIR, *Middleſhaw, near Kendal, Nov.* 26, 1802.

THE ſubſtance called Caoutchouc, or Indian Rubber, poſſeſſes a ſingular property; which, I believe, has never been taken notice of in print, at leaſt by any Engliſh writer; the preſent letter contains my experiments and reflections on the ſubject; and ſhould they appear to deſerve the attention of your philoſophical friends, I am certain you will take the trouble of communicating the paper to the Literary and Philoſophical Society of Mancheſter.

The property I am about to deſcribe depends on the temperature of the Caoutchouc, which is uſed in the experiment; for heat increaſes the pliancy of the ſubſtance, and cold, on the contrary, renders it more rigid: ſo that when a ſlip of this reſin has been ſufficiently warmed, it may be extended to more than twice its natural length, by a moderate force applied to its extremities, after which it will recover its original dimenſions in a moment, provided one of the ends of it be let go as ſoon as it has been ſtretched. This diſpoſition of the ſubſtance may be produced by a degree of temperature leſs than the heat of the blood; it is therefore neceſſary to prepare a ſlip of it, by ſteeping it for a few minutes in warm water, or by holding it ſomewhat longer in the fiſt; either of theſe precautions makes the reſin pliant, and fits it for the experiment; which is performed in the following manner.

Caoutchouc more pliant by heat,

* Mancheſter Mem. N. S. Vol. I.

I made

and less dense. I made a piece of Caoutchouc a little heavier than an equal bulk of water, the temperature of which was 45 degrees: the vessel containing the resin and water was then placed on the fire; and when the contents of it were heated to 130 degrees, the Caoutchouc floated on the surface.

It becomes cold by sudden drawing out, and hot by contraction. *Exp.* 1. Hold one end of the slip, thus prepared, between the thumb and fore-finger of each hand; bring the middle of the piece into slight contact with the edges of the lips;* taking care to keep it straight at the time, but not to stretch it much beyond its natural length: after taking these preparatory steps, extend the slip suddenly; and you will immediately perceive a sensation of warmth in that part of the mouth which touches it, arising from an augmentation of temperature in the Caoutchouc: for this resin evidently grows warmer the further it is extended; and the edges of the lips possess a high degree of sensibility, which enables them to discover these changes with greater facility than other parts of the body. The increase of temperature, which is perceived upon extending a piece of Caoutchouc, may be destroyed in an instant, by permitting the slip to contract again; which it will do quickly by virtue of its own spring, as oft as the stretching forth ceases to act as soon as it has been fully exerted. Perhaps it will be said, that the preceding experiment is conducted in a negligent manner; that a person, who wishes for accuracy, will not trust his own sense of feeling in inquiries of this description, but will contrive to employ a thermometer in the business. Should the objection be started, the answer to it is obvious; for the experiment in its present state demonstrates the reality of a singular fact; by convincing that sense, which is the only direct judge in the case, that the temperature of a piece of Caoutchouc may be changed, by compelling it to change its dimensions. The use of a thermometer determines the relative magnitudes of these variations, by referring the question of temperature to the eye; experiments of this sort are therefore of a mathematical nature, and afford a kind of knowledge with which we have nothing to do at present; for we are not inquiring after proportions, but endeavouring to esta-

* This effect was first noticed in 1784, at Mr. Kirwan's meetings in Newman street, and Dr. Crawford ascribed it to change of capacity similar to what he supposed to take place in a nail by hammering.—N.

blish

blish the certainty of a fact, which may aſſiſt in diſcovering the reaſon of the uncommon elaſticity obſervable in Caoutchouc. My eſſay or letter appears to be running into a long digreſſion; the ſubject muſt therefore be reſumed, and it will not be improper to premiſe the following ſimple experiment, in the preſent ſtate of the inquiry; becauſe it ſeems capable of affording no inconſiderable degree of inſight into the plan which nature purſues in producing the phenomenon in queſtion.

Exp. 2. If one end of a ſlip of Caoutchouc be faſtened to a rod of metal or wood, and a weight be fixed to the other extremity, in order to keep it in a vertical poſition; the thong will be found to become ſhorter with heat and longer with cold. The proceſſes of heating, cooling, and meaſuring bodies are ſo well known, that I need not enter into the minuter parts of the experiment; it will be proper, however, to add, that an increaſe of temperature diminiſhes the ſpecific gravity of the Indian Rubber, and a loſs of heat occaſions a contrary effect in it; as I have proved experimentally. The knowledge of the latter fact leads me to conclude, apparently on reaſonable grounds, that the pores or interſtices of Caoutchouc are enlarged by heat, and diminiſhed by cold; conſequently when a ſlip of this ſubſtance which remains extended by a weight, or the application of force, happens to contract from an acceſſion of temperature, the capacity of its pores, taken ſeparately or collectively, is augmented by the change that takes place in the figure of the thong. Now if the exiſtence of caloric be admitted, it will follow from the preceding arguments, that the phenomenon under conſideration is occaſioned by the alternate abſorption and emiſſion of the calorific fluid, in the ſame manner that ropes, the blades of Fuci, as well as many more bodies, are obliged to contract and extend themſelves, by the alternate abſorption and emiſſion of water.—You will perceive by the tenour of the foregoing obſervations, that my theory of this caſe of elaſticity is perfectly mechanical; in fact, the explanation of it depends upon the mutual attraction of Caloric and Caoutchouc; the former of which penetrates the latter, and pervades every part of it with the greateſt eaſe and expedition; by which the reſin is compelled to accommodate its pores to that portion of the Calorific fluid which is due to its whole maſs,

Caoutchouc when ſtretched expands by heat and contracts by cold.

Theory. That this ſubſtance is affected by caloric, as ropes are by water

at

at any particular degree of temperature. In order to apply the last remark to the phenomenon under consideration I may observe, that if a force be exerted on a piece of Caoutchouc to alter the dimensions of its pores, the mutual attraction mentioned above will resist the effort. But the ease with which this substance may be made to change its figure, and the retractile power which it possesses on these occasions, shew that its constituent particles move freely amongst themselves: but where there is motion, there is void space; consequently Caoutchouc abounds with innumerable pores or interstices, the magnitudes of which are variable, because the specific gravity of the resin becomes less with heat, and greater with cold. Now if the dimensions of the pores in a piece of Caoutchouc can be lessened, without taking away part of the matter of heat, which it contains at the time; this new arrangement in the internal structure of the slip will lessen its capacity for the matter of heat, and consequently augment its temperature. But the warmth of such a slip is increased by stretching it, according to the first experiment; the pores of it are therefore diminished: and the effort, which it exerts at the time, arises from the mutual attraction of the Caoutchouc and Caloric; which attraction causes an endeavour to enlarge the interstices of the former for the reception of the latter; hence it happens that the thong contracts longitudinally, according to the second experiment, and the redundant caloric is absorbed in the course of this operation, which again reduces the temperature. The preceding explanation agrees very well with the phenomenon, as it is stated in the beginning of this letter; and the theory receives additional confirmation from the following facts.

that its capacity may be mechanically altered and the caloric extruded, &c.

Exp. 3. If a thong of Caoutchouc be stretched in water warmer than itself, it retains its elasticity unimpaired; on the contrary, if the experiment be made in water colder than itself, it loses part of its retractile power, being unable to recover its former figure; but let the thong be placed in hot water, while it remains extended for want of spring, and the heat will immediately make it contract briskly. The foregoing circumstances may be considered as proving, that the elasticity of Caoutchouc is not a constitutional quality of the substance, but a contingent effect, arising from the loss of equilibrium between the portion of caloric, which the resin

Overstretched Caoutchouc does not completely recover itself in the cold; but heat restores its elasticity:

whence the nature of its elasticity is deduced, &c.

vessel happens to contain at any moment, and its capacity to receive that fluid at the same instant. The object of the present letter is to demonstrate, that the faculty of this body to absorb the calorific principle, may be lessened, by forcibly diminishing the magnitudes of its pores; and this essential point of the theory may be confirmed by experiment: for the specific gravity of a slip of Caoutchouc is increased, by keeping it extended, while it is weighed in water.

JOHN GOUGH.

VIII.

Observations on the training of Pugilists, Wrestlers, Jockies, and others, who give themselves up to Athletic Exercises; with some Queries for discovering the Principles thereof, and the Process of training Running Horses, &c. with a View of ascertaining whether the same can furnish any Hints serviceable to the Human Species. *

PROFESSIONAL men are ready to acknowledge, that prevention is better than cure; and the best informed ingenuously admit, that organic diseases, once confirmed, are beyond the reach of their art. As organic diseases generally proceed from slow and gradual changes, they may certainly be prevented by temperance and labour; by activity of body, and contentment of mind. In regard to the common metaphysical expressions, "of the exhausting of the excitability; of the wearing of the parts; of the attrition of our fluids, in circulation, against the solids; of the abrasion of the solids by fric- General considerations on organic diseases.

* The subsequent queries and observations have been circulated by Sir John Sinclair, with a view to obtain information concerning the effects of diet and exercise on the human frame, from a class of practical experimentalists, whom the pride of science has hitherto overlooked. The philosophical manner in which this branch of dietatic medicine is here considered, appears to render it a fit object for insertion in a Journal conducted on the plan of the present. In promoting the circulation of this paper, we have no doubt that we are coinciding with the plan of the author, by extending his means of information: Any communications tending to throw further light on the subject, will be acceptable. W. N.

 tion;

tion; of the debility produced by the most natural powers supporting life, namely, the waste of substance created by that exercise and labour, for which we seem peculiarly destined,"—all these expressions are extremely suspicious. The speculator is always to be suspected, when, forsaking plain direct facts, he involves his want of meaning, and conscious ignorance, in learned words, or metaphor.

It is usually supposed that bones, and other solid parts are permanent; but they are successively replaced, like the fluids.

These metaphorical expressions have originated in a persuasion, that the bones, cartilages, muscles, and other solid parts, being once formed, *are permanent*, because the identity of the individual is permanent; and that being once formed, and always retaining one shape, their actual component parts must continue the same. Nothing in philosophy is farther from the truth. There are experiments to demonstrate, that every part and particle of the firmest bones, is successively absorbed and deposited again*. The solids of the body, whatever their form or texture, are incessantly renewed. The whole body is a perpetual secretion, and the bones and their ligaments, the muscles and their tendons, all the finer and all the more flexible parts of the body, are as continually renewed, and as properly a secretion, as the saliva that flows from the mouth, or the moisture that bedews the surface. The health of all the parts, and their soundness of structure, depends on this perpetual absorption, and perpetual renovation; and exercise, by promoting at once absorption and secretion, promotes life, without hurrying it; renovates all the parts and organs, and preserves them apt and fit for every office.

Nutrition is a general process.

Nutrition belongs not to the stomach alone, which but prepares the food, and converts it into chyle, but to the vessels by which it is circulated, and appropriated to the nutrition of parts, which of course is performed by every petty artery of the body.

Many general rules are rash and dangerous.

In nothing should we be more anxiously careful, than in laying down rules, which must affect the health of thousands; and whenever we proceed on doctrines, unsupported by fact,

* This has been ascertained by giving madder to growing animals, especially pigs and fowls, among their food. It is found that the madder tinges the bones, layer after layer, with a red colour; and by the deepness of the tinge, demonstrates the succession in which the particles of the bone are absorbed and deposited. This is, I believe, the conclusion which physiologists have formed.

wherever

wherever we divert mankind from those amusements and labours to which nature excites us, we should proceed with particular caution. We read in books, that life and the body are but as a given quantity of living energy and living materials, to be expended and used with discretion and economy; and that the sum of excitability, which is born with the child, is expended towards the close of life. The doctrine of abrasion also intimates, that our solids are perpetually wasting, and that it is by the diminution of moisture,—the aridity of solids, the scantiness of fluids, and the slow induration of the solid parts; that the body becomes shrunk, emaciated, stiff, and motionless, before it sinks into the grave. And, rash as the doctrine seems, it has been boldly asserted, that "to live with as little food, and as little exercise as possible, is the surest means to preserve the body, and to live long." To live with as little food, and as little exercise as possible, would make a man little better than a mere grasshopper. A man living thus, would be a voluntary prisoner, wan, colourless, fleshless, bloodless, having no speculation in his eyes, no marrow in his bones; his complexion would declare him what he was. This system practised, either in infancy, in the prime of manhood, or in the decline of life, would abridge it. Ascetics are a proof, not of the length of life, which temperance insures, but of the premature old age which abstinence brings upon us. The squalid look, the hollow cheek, the matted hair, the emaciated body, only prove how much, by such criminal self-denial, the body suffers, with but little profit to the powers of the mind. Let us then take care that our philosophy be not too severe; for men may run into real danger, if we take from them every fair indulgence, or divert them from following the dictates of nature. The fairest livers, who have not abused, but have enjoyed their strength and health, have in general enjoyed them longest.

The doctrine of abrasion or wearing out has been absurdly applied.

There are habits which seem to be natural to, and congenial with, the several periods of life. The child should merely suck, sleep, and vegetate. The boy should ramble wild and unconstrained, little oppressed with tasks or studies, and nourished with abundance of simple food. The youth should be temperate, sober, active. The old man quiet, sedate, self-indulgent; should have long sleep, delicate food, rich wines, and agreeable temperature; little labour, and a cheerful mind.

Natural habits of infancy, youth, &c. &c.

Nature assigns us vigour, spirit, enterprise, and foresight in the early part of life, to treasure up the needfull indulgences for age. Parents are careful of our first infancy; we ourselves ought to provide for our latter childhood.

Considerations respecting the functions of the skin;— The most intelligent professional men have an opinion concerning the functions of the skin, consonant with that of the vulgar; and more refined, only from their assigning a general cause for those effects, of which all of us are conscious. The skin is not regarded merely as an organ of secretion, destined for draining off superfluous moisture, or saline particles, from the general mass of fluids, but as a surface of more active circulation, which solicits the blood to the very extremities of the vessels, and thus contributes to support and complete the circulation of the blood, and to nourish the parts within. The skin is regarded as connected, in a peculiar manner, with all the parts of the cellular substance, interposed betwixt the muscles, and involving the blood vessels. The state of the skin indicates the condition of that cellular substance, whose office it is to conduct the blood-vessels to all parts, especially to the muscular flesh, and to nourish the parts; and while the circulation of the skin is lively and active, that of the involved parts can never flag. —and the intestines. The condition of the bowels, and of the skin, are the first and most natural points for the physician to attend to. It is by regulating these, that he regulates the pulse; by stimulating or soothing them, that he raises or depresses the vital actions; and it is matter of common observation, that in animals, a good skin is the criterion of health, and the dryness of the skin, the forming of scabs or eruptions upon it, and the clapping of the hair, (as it is called by those who have the care of stock), are the first and surest signs of approaching disease.

The lungs and their office. Next to the free circulation of the blood through all the body, terminating in the surface, that of the free transit of the blood through the lungs, is essential to health.

The oxydation or chemical change produced by air upon the blood, is essential to its vital properties. A free and powerful respiration is most essential to a fresh colour of the face, to lively spirits, and cheerful feelings, and to the healthy and vigorous actions of the body. "It is my breathing hour of the day," says Hamlet to Osric. It is a princely thing to set apart hours for exercises; and there is little doubt, that if all those

those, who linger away their hours in luxurious and indolent relaxations, were to assign a regular portion of their time to the hardy and manly exercises of walking, riding, fencing, &c. and would take *their breathing hour*, they would breathe long and well.

These reflections naturally arise upon considering the almost incredible perfection, to which those, whose profession it is to train men to athletic exercises, have brought their respective arts. By certain processes, they improve the breath, the strength, and the courage of those they take in hand, so as to enable them to run thirty, or walk a hundred miles, in a given space of time; to excel in wrestling; or to challenge a professed boxer. Would it not then be a most important addition to the facts we already know concerning the means of improving strength, and ensuring long life, if authentic information could be procured from those districts where athletic exercises prevail, what are esteemed the best and surest processes for training men for foot-races, trials of strength in wrestling or boxing matches, or for raising the strength and courage of game-cocks, or improving the wind, strength, and speed of running horses to their highest pitch.*

The art of training men to athletic exercises is wonderfully effective.

Those who give themselves out as skilful in this art, attend to the state of the bowels, the skin, and the lungs. They use such means as reduce the cellular or fatty substance, and invigorate the muscular fibres. When they take a man in training for any feat of this kind, he is not oiled and suppled as the ancient athletics were; for as their common modes of life were hardy and active, they needed no other preparation: but he is sweated, purged, and dieted, and then put upon trial. He is purged with very drastic purges, to reduce his grossness. He is made to walk out under a load of clothes; his walks are regularly increased, and a certain number of times a-week; he is laid between two feather-beds; sweat promoted by drinks; his limbs taken from between the feather-beds, successively, and rubbed very roughly. After enduring for many

Some account of the methods,

* Though not immediately connected with the object of this paper, it may not be improper to suggest, that it would be of great importance, if medical gentlemen, whether in the army or navy, who have been on service, were also to point out the various circumstances which tended to support, or to abate, the strength and courage of the soldier or the sailor.

hours

hours this ſtate of ſuffocation, he is comforted with a draught of ale or wine. The purges and ſweatings are repeated, according to the groſſneſs of his habit, and from time to time his trainer, (regarding him no otherwiſe than he would a running horſe, under the like diſcipline,) takes him out, and makes trial of his wind and ſtrength, and does not ceaſe till he has made him as lank as a greyhound, and almoſt as fleet.

and the great increaſe of force acquired by the human frame.

A man, even in the beſt of ordinary health, becomes giddy and breathleſs when he ſtrikes; and ſick and pale on receiving a few blows. He is thence unable to bear any unuſual exertion, and by inference prone to diſeaſe. If, by extenuating the fat, emptying in the cellular ſubſtance, hardening the muſcular fibres, and improving the breath, a man of the ordinary frame may be made to fight for one hour, with the utmoſt exertion of ſtrength and courage; the inquiry which I have already ſuggeſted muſt be of the higheſt uſe. For were this new train of facts regularly laid before profeſſional men, and were they enabled thus to judge of the influence which the methods of theſe practical philoſophers have on regulating the functions of breathing, perſpiration and digeſtion; it would be drawing into the province of ſcience, an art connected moſt particularly with the means of prolonging life, and hitherto known and practiſed only by a few inſulated individuals, of courſe imperfectly known, and of too limited uſe.

Theſe facts are probably of great value to the ſcience of prolonging life.

The art ſeems to be modern.

I queſtion whether the athletics of old uſed ſimilar means; whether they were equally ſucceſsful; whether there ever were, in any climate, age, or country, more hardy or powerful frames than thoſe of our Engliſh pugiliſts. In Cooke's voyage, we are told of the marked inferiority of the Engliſh ſailors, in wreſtling or boxing, to the naked ſun-burnt heroes of the South Sea Iſlands. But an Engliſh ſailor, though full of ſpirit and vigour, is as clumſy as a clown, and could not even row againſt an inhabitant of the Sandwich Iſlands. An Engliſh bricklayer, blackſmith, or drayman, however, who liked the ſport, and was practiſed in balancing and ſtriking, might have challenged the whole of the tawny nation.

Queries.

With a view of collecting ſuch important information, I am very anxious that the following queries ſhould be propoſed to theſe who profeſs the art of training pugiliſts, wreſtlers, and runners of foot-races, by ſuch intelligent men as have the opportunity of converſing with them.

1. By

1. By what criterions or they judge of the mufcular ftrength, or wind, or other alities of thofe who feek to put themfelves under training. What is the earlieft, and what is the lateft age they would attempt to train? Tefts of ftrength &c.?

2. How they judge of the length of time that may be required for bringing a man into good plight, vigorous health, and free breathing; and what period of preparation is ufually required for running a match? Time required to train?

3. What purges they ufe; and in what fucceffion; and by what rules do they adminifter them; and how do they judge of their effects? Is the purging only preparatory, or is it regularly continued? Is it meant, by this procefs to reduce the plethoric ftate of the fyftem, (on the idea that there is too great a quantity of blood,) or is it fimply defigned to put the bowels in the moft favourable condition, for eafy and good digeftion? Is the reducing the actual fize of the belly, neceffary to more free and perfect breathing *? Purges, treatment, their object, &c.?

4. Is the diet rich or fimple; of animal food, or of vegetable; in great quantity, or fparing; is it increafed gradually, or diminifhed gradually? What meals have they in the day; and at what hours; one or more; frequent feeding, in fmall and fixed portions, or full and fubftantial meals? What kinds of flefh or meat is reckoned the beft; whether beef, mutton, veal, pork, lamb, or fowl? Are any kinds of fifh allowed? What quality of food is moft conducive to ftrength? What quantity is neceffary for maintaining the fyftem in its moft perfect ftate of vigour? Do they feed much in the intermediate days of the purges? Is abftinence required when they take their phyfic? Diet?

5. What kinds of liquors are reckoned beft? Whether wine, ale, water, fpirits, &c.? Whether given hot or cold; in what quantities; and when ought they to be given? Liquors?

* The effects of taking up a running horfe from idlenefs and foft pafture, to hard food and regular exercife, is attended with this peculiar effect, that while the animal becomes lank, fleck, and gloffy, while he gets fire in his eye, and a new vigour in his limbs, and wind and fpeed, his belly, (fwollen with coarfe indigeftible food, eaten in great profufion,) is drawn into half its fize. May we not then prefume from this analogy, that the ftate of the belly has a remarkable effect upon the wind.

3 6. Are

Intention of the perspirations, how excited, &c.?

6. Are the very violent perspirations into which they throw their patients, designed to reduce the system, to extenuate the fat, to lessen that quantity of blood, the excess of which makes us giddy or short breathed; or is it merely designed to produce a new condition of the skin, more favourable to health and muscular vigour; to produce a sharper appetite; a greater demand for food; and a quicker nourishment, or a greater nutrition from a more slender diet? Is the sweat at first produced by exercise, and only continued by the person, when trained, being put between feather beds, and encouraged by drinks; or is it produced by force of sweating drugs, or violent heats, or by continued friction? At what hours are the perspirations brought on? How is the pupil treated when the sweat is over? What becomes of the skin of a fat man, when, by the process, he is reduced in size, and rendered lean? Does it hang loose, or is it tight? Has it any effect upon the bones?

Exercise and treatment?

7. What hours of exercise do they require of their pupils during the day? At what hours do they send them out in the morning? How long do they continue abroad? Are they loaded with clothes after the body is reduced, and becomes limber, and thin and muscular; or only while the sweating process continues? Are they fed before they go abroad, or when they return? What trials are made of their strength? When is a man known to be up to his full strength and breath in training? At what hours do they go to bed? What sleep are they allowed? What indispositions are they subject to during training? Are there any circumstances by which the process may be interrupted; or any circumstances, in consequence of which, it must sometimes be abandoned?

Subsequent effects of training?

8. What is the state of the health, after they give up training? Are they subject to any complaints; and what are they? How long does the acquired excess of strength continue?

What part of training most effectual? Whether it be permanent, temporary, curative

9. It is most interesting to learn, on which part of this process, the purging, the sweating, the exercise, or the feeding, they most depend; and whether it procures a permanent increase of vigour, easily maintained by suitable diet and exercises, or only a temporary excitement, calculated for the particular occasion? Also, whether persons have ever thought of undergoing this process, not for the purpose of running matches, but to recover health; with what success

this

this has been done, and whether it is to be recommended for gout, corpulency, asthma, nervous disorder, or other maladies, as likely to be of service?

These are questions, of the importance of which, those who are best able to answer, may not be fully aware. But nothing which so suddenly changes the powers, and the very form and character of the body, from gross to lean, from weakness to vigorous health, from a breathless and bloated carcase, to one active and untiring, can ever be unimportant, either to the art of physic in general, or to that branch of it more immediately connected with inquiries regarding health and longevity. The art must be of importance.

The queries to be put regarding jockies, running-horses, or game-cocks, may be to the following effect:

1. *Jockies.*

1. What is the process used in training them, and reducing their weight? Queries respecting Jockies.

2. What effect has it upon their health and strength?

3. What effect has it upon their mind, in regard to courage, quickness, &c.

4. How long do these effects continue?

5. After being reduced, do they quickly get fat again, or do they continue long in the state to which they were brought?

6. Are jockies, accustomed to be thus treated, healthy and long lived?

2. *Running Horses.*

1. What are the principal objects to be attended to in regard to running-horses? Do their perfections depend upon parentage, and whether most upon the male or the female? Is it necessary that the mare should have gone her full time, to bring a perfect foal? Is the gradual growth of the foal essential? Is there a great difference, in regard to natural constitution, between horses of the same parentage? What kind of form is in general preferred? Do you prefer great or small bones? Which sex is preferable for speed, and which for strength? Running horses.

2. What is the best age for beginning to train horses for the turf? Are they first put upon grass? What is the effect of soft

foft meat? When fhould they be put on hard meat? What are the effects thereof? Is it neceffary to purge them frequently? Have the purges any tendency to weaken them? What food is reckoned the moft nourifhing? How often are they fed? What drinks are given them, and how often? Whether hot or cold? Is it neceffary to keep their fkin perfectly clean, and how? Is it neceffary to make them perfpire much? What exercife is given them? How is the training completed?

3. After the training is completed, can the perfections thereby obtained be eafily kept up? Does the procefs effect merely a temporary change, or does it laft during life? Are running horfes as long lived as others, or do they foon wear out?

3. *Game-Cocks.*

Game-cocks. 1. Does the fuperiority of game-cocks depend upon parentage? Which is of moft importance, the male or the female? Is it of any confequence that the cock fhould arrive rather gradually at maturity? Is there a great difference, in point of ftrength and conftitution, in game cocks of the fame parentage? Do you prefer great or fmall bones?

2. When do you begin to feed the young cocks? What diet and drink do you give them, and what is the procefs by which they are brought to the greateft poffible height of ftrength and fpirit?

3. When the game-cocks are thus trained, how long do the effects thereof laft? Are they temporary or permanent? Do game-cocks thus trained live fhorter or longer than others of the fame fpecies?

4. What drugs are given to fighting-cocks immediately before the main begins? Is it not ufual, by giving them faffron, (or fome drug which has the fame effect with opium, as ufed among the Janifaries, or brandy among the French foldiery,) to excite an unnatural and fhort-lived courage? What are the effects of fuch drugs? and how do they manage the feeding up to this point, fo as to take advantage of this momentary excitement?

On the Dangers encountered in travelling over Downs, occasioned by Quickfands, which are frequently found on the Sea Coast; with an Indication of the Means of avoiding them. By M. Biemontier, *Inspector-General of Bridges and Roads.**

AFTER heavy and continued rains, there are formed at the edge of the fea-downs, fmall pools, or collections of water, frequently of feveral feet in depth. Strong winds diflodge portions of fand from the general mafs, and tranfport them to a diftance; which falling in fhowers on the clayey and fheltered furface of thefe pools, defcend gradually, and remain as it were in equilibrium in the midft of the water, fo as to form an infinity of little vaulted cavities. Thefe arches fuftain others, which are again furmounted in a fimilar manner, till at length the mafs rifes, fometimes to feveral feet above the level of the water; the furface becomes white and dry, and the fnare lies perfectly concealed. Whoever walks over this ftructure deftroys the whole, the arches give way, and the intruder is immerfed fometimes to his waift; but his alarm is ufually greater than the real danger; for if he were buried even up to the neck, he might eafily extricate himfelf, only by retaining fufficient prefence of mind not to ftruggle, but to move flowly and deliberately; want of attention to this might hazard his deftruction.

Quickfands formed by bodies of fand tranfported by the wind into pools of water, where they form vaulted cavities till the hollow of the pool is filled up.

The furface is dry and fcarcely differs in appearance from the contiguous ground, but it gives way when trod upon.

When the equilibrium of the maffes of fand is deftroyed, they naturally fall into heaps, and it is only neceffary that time fhould be allowed for this to take place. When this has happened, the perfon immerfed fhould gently lift up one leg, and remain in that pofition till the fand has formed a fufficient bottom to fupport his raifed foot; the other leg fhould then be lifted up with the fame precautions; and thus fucceffively, till he rifes to the furface. In the mean time, the water which had been confined in the hollows of the fand will have alfo rifen, forming a pond (three or four inches deep) through which the adventurer may pafs in perfect fafety.

Management to avoid danger.

* Bibliotheque Phyfico-Economique, &c. de Sonini, November, 1805, page 186.

Cows,

Animals when immerfed ufe the fame method;

Cows, dogs, and other animals who frequent downs, and chance to fall into thefe quickfands, either through inftinct or experience, make ufe of this method to regain their freedom; provided, however, they be not too deeply immerfed to retain the free ufe of their fhoulder joints, otherwife they cannot be extricated without affiftance. I experienced this twice in one day; my horfe fank to above the breaft-leather, and although he was very ftrong, his efforts to extricate himfelf were unavailing, till we had removed fo much of the fand as impeded the action of thofe joints.

but they are feldom caught in quickfands.

It rarely happens that animals accuftomed to live on downs are caught in thefe fnares, which they are aware of, and know how to avoid.

Inftance.

I attempted, but in vain, to force another horfe with the whip and fpur into a quickfand; his owner, who acted as guide, affured me, that I fhould not fucceed, although there was no other indication of the fpot than a flat furface, flightly wrinkled. By thefe marks the traveller may generally detect the concealed pitfall; but he may always avoid them by tracing the footfteps of the cattle, when vifible, or by walking a few fathoms above the bottom of the declivity, or on the fummit of the down.

Another kind of quickfand,

Another kind of quickfand is fometimes met with on the fea-fhore, between high and low water mark, which it is proper fhould be here taken notice of. This is fometimes the effect of rain, but more commonly of the fea, when forced by wind and tempeft beyond its ufual limits, which being generally more elevated than the diftant land, the waters thus impelled forward are prevented from returning to their ancient bed; they therefore after forming in a body, drain away through the earth they have inundated, or brought with them, and form excavations beneath, large or fmall, deep or fhallow, according to circumftances.

formed by the waters drained through the earth.

I ought not to omit a fingular fact which paffed under my own obfervation, and which feems to prove, (as I have already ftated) that animals frequenting thefe plains, and living near the borders of the fea, employ combined means, acquired undoubtedly by experience, to extricate themfelves from thefe cavities, wherein they muft inevitably perifh, did they, as it appears natural they fhould, attempt to efcape by recoiling or by flight.

 Traverfing

Traverfing the plain of Arcachon, after a violent tempeft, which had been accompanied with heavy rains, we thought it prudent to get off our horfes and lead them by the bridle. One of the horfes who was left to himfelf, immediately quitted the company; and was retiring from the fhore, but being compelled to return by the application of the whip, he went upon the quickfand, which probably he had attempted to avoid by defertion; but the moment he felt the earth giving way, he crouched down, or rather threw himfelf precipitately on his fide. The ground quickly fank beneath and round about him; the water furmounted the fand; the horfe was only wetted to the crupper, and we efcaped with no other damage than the lofs of our ftock of bread, which being foaked in the falt water was rendered unfit to be eaten. Singular incident.

It may be received as matter of fact, that a man who fhould experience a fimilar misfortune, could not do better than to extend himfelf in the like manner, nearly in the attitude of a fwimmer, when he throws himfelf into the water. It is fcarcely neceffary to explain the fuperior advantage of this method; a plate of lead, of fome thoufands of weight, and feveral feet in breadth, if caft flat into any liquid body, would reach the bottom no quicker than the fluid could efcape to make way for it; if a fimilar body were to fall upon a quickfand, it would fhake every part of it, but would prevent the fand or earth from rifing, while the firm furrounding earth would confine it laterally: the ruins of the arched vaults would replace the waters which had been liberated from their fubterraneous confinement; folid heaps would then neceffarily be formed towards the centre, and the incumbent body would remain at the furface, or at leaft it would not be fwallowed up. Travellers when caught in this fnare fhould throw themfelves down.

Thefe quickfands are generally denoted by fmall ftreams, below which, when practicable, there is no danger in paffing. Quickfands are denoted by rills of water.

X. Extract

X.

Extract from a Memoir by Messrs. FOURCROY *and* VAUQUELIN, *on the Guano, or Natural Manure, of the small Islands of the South Sea, near the Coast of Peru. Read at the French National Institute, by* A. LAUGIER.*

M. Humboldt the first who gave an account of the Guano.

AMONG the multitude of subjects worthy the attention of the naturalist, which the philosophical Humboldt observed and collected during his travels, the Guano is not the least considerable, from the interest which it excites. This celebrated naturalist, by making us acquainted with this singular matter, one of the principal resources of agriculture in the countries he visited, has given confirmation to a discovery made by the authors of this memoir, about the time of his return. Reading their memoir on the existence of uric acid in the excrements of birds, it occurred to him that the Guano of the islets on the coast of Peru, which are frequented by great numbers of birds, might possibly be of the same nature. It remained for chemical investigation to examine how far this conjecture was well founded; and Messrs. Fourcroy and Vauquelin undertook the analysis of this matter. The following is the result of their labours, with this view, extracted from the Memoirs of the National Institute.

Memoir by Messrs. Fourcroy and Vauquelin on the excrements of birds, suggested the notion that the Guano was derived from the same origin.

Before I enter upon a detail of the experiments made upon Guano, in order to ascertain its nature, it may not be irrelevant to the subject to transcribe what M. Humboldt himself says of this substance in a note sent to the authors of this memoir.

Extract from M. Humboldt's note.

Guano found on certain small islands,

"The Guano is found in abundance in the South Sea, in the Chinche islands, near Pisco; and also on the more southern coasts and islets of Ilo, Iza, and Arica. The inhabitants of Chancay, who make Guano an object of their commerce, go to and return from the Chinche islands once in 20 days.—Each vessel contains from 1500 to 2000 cubic feet. A vanega sells at Chancay for 14 livres, and at Arica for 15 livres, Tournois.

—in beds 50 or 60 feet thick.

"Guano is dug from beds 50 or 60 feet thick; where it is worked like the bog-ore of iron. The islets where it is found

* Annales de Chimie, Vol. LVI. p. 258.

are frequented by a multitude of birds, particularly of the species of Ardea and Phœnicopterus, who roost there every night: but the excrements of these birds have hardly formed, in three centuries, a layer of four or five lines in depth. Is then the Guano the effect of some convulsion of the globe, like pit-coal and fossil wood? The fertility of the naturally sterile soil of Peru is derived from the Guano, which has become a material article of commerce. Fifty little vessels, called *Guaneras*, are constantly employed in fetching this manure, for the supply of the coast. Its effluvium may be smelled at the distance of a quarter of a league. The sailors accustomed to this smell of ammonia, feel no inconvenience from it; but we could not approach it without being affected with continued fits of sneezing.

The place frequented by vast numbers of birds.

Sterile soil of Peru made fruitful by the Guano.

Vessels employed to collect it.

It has a strong odour of ammonia.

" Maize is the particular vegetable for which Guano forms an excellent manure. The Spaniards learned its use of the Indians: If too much be thrown upon the maize, the root is burned and destroyed. Guano is too acidifiable, and is therefore a manure containing hydruret of azote; whilst all other manures are rather hydrurets of carbon."

Maize particularly benefited by Guano as a manure.

Guano is of a dirty yellow colour, rather insipid to the taste, but possessing a powerful odour, partaking of castor and of valerian. It turns black in the fire, and exhales a white smoke of an ammoniacal smell.

Its appearance.

Its solubility in water, particularly with potash, determined the operators as to the method they should pursue in its analysis. They treated it successively with water, with potash, and with muriatic acid; each of which methods presented many phenomena, as related in the following part of this paper, divested of the particular details of process, which are too extensive for an extract.

Partly soluble in water.

Ten grammes of this matter, after being repeatedly washed with large quantities of boiling water, were reduced to $5\frac{7}{10}$ grammes. The water had obtained a red colour, which it communicated to paper stained with turnsole.

The solution is acid.

In distillation, the water yielded ammonia during the whole operation. Twenty-four hours afterwards, it had deposited a dirty yellow powder, possessing very little flavour, but with an odour of castor: On the surface was a crystalline pellicle, of the same colour with the deposition.

The water yielded ammonia by distillation, and deposited a yellow powder with a smell of castor.

The

The liquor, filtered and again evaporated, till reduced to 3 grammes, on cooling again, depofited a fawn-coloured powder, fimilar to the former, but in lefs quantity.

The powder, and the mother-water, which had held it in folution, were feparately examined.

Examination of the powder.

The powder offered the following properties:—It is a concrete and pulverulent fubftance, of a brilliant cryftalline afpect, and of a dull yellow colour. Before the blow-pipe it is confumed entirely away, yielding a flight empyreumatic odour of ammonia and pruffic acid. It is very little foluble in cold water; but abundantly fo in warm water, to which it communicates its yellowifh colour. This folution, though taftelefs, ftrongly reddens the tincture of turnfole, precipitates folutions of acetate of lead, and of nitrate of filver and mercury, in coloured flakes, which are readily and completely rediffolved by nitric acid.

This matter inftantaneoufly diffolves in an alkaline ley, which it tinges of a deep brown colour, exhaling a pungent fmell of ammonia. Sulphuric acid poured into the concentrated alkaline folution, throws down a very thick whitifh precipitate, and difengages a brifk odour, refembling that of weak acetic acid.

It is an acidulous falt, compofed of animal acid, ammonia, and lime.

The learned authors of this memoir conclude from their experiments, that this powder is an acidulous falt, compofed of animal acid, ammonia, and a little lime. In fact, very weak nitric acid, wherein this falt had been macerated in order to difengage the acid it contained, from its bafes, yielded, on evaporation, copious ammoniacal vapours, by the addition of potafh, and unequivocal figns of the prefence of lime, by the addition of oxalic acid.

Analyfis of the powder when deprived of its ammonia and lime.

When thus deprived of its ammonia and lime, this matter is lefs coloured and lefs foluble than before. Its folution in boiling water depofits pretty hard and brilliant cryftals, and more deeply reddens turnfole paper. It combines readily, and without any ammoniacal vapour, with potafh, from which all the acids again feparate it. Heat turns it black; and it burns, without leaving any refiduum, with an odour of ammonia and of pruffic acid. A neutral combination of it with ammonia will not precipitate the folution of fulphate of alumine, as is done by homftic acid.

From these facts it appears evident, 1, that the matter taken up by the boiling water from Guano is an acid, partly saturated with ammonia and a little lime; 2, that this acid is an animal product, because it yields ammonia and prussic acid, when decomposed by fire; 3, that the same acid, according to all the known properties, must be uric acid, similar to that contained in the excrements of aquatic birds; 4, that it forms about one fourth part of the Guano.

The acid of Guano is uric acid, forming about ¼ of the whole.

The mother-water which deposited the powder, whose qualities have been just examined, is very acid; potash causes a copious disengagement of ammonia: It contains, therefore, an ammoniacal salt. Nitrate of barytes and of silver announce the presence of muriatic and sulphuric salts; which are precipitated in white flakes by lime-water, and are re-dissolved, though with difficulty, in muriatic acid.

Analysis of the mother-water which deposited the powder. It contains an ammoniacal salt,

This precipitate caused by lime water, is evidently formed of two salts, both soluble in acids without effervescence; one easily, and without the assistance of heat, the other with difficulty, even with the aid of heat; the former resists calcination, the latter is decomposed by fire, and afterwards dissolves in acids with effervescence. The first is phosphate of lime, the second oxalate of lime.

—and phosphate and oxalate of lime,

Messrs. Fourcroy and Vauquelin wished to separate these two salts, without their undergoing any alteration; and with this view they made use of weak nitric acid, which dissolved the phosphate of lime, and left the oxalate untouched. The latter salt, on being treated with a solution of carbonate of potash, yielded a precipitate that dissolved with effervescence in nitric acid: This solution displayed all the properties of nitrate of lime. The acid separated from the lime was taken up by the potash: in fact, the liquor possessed the characters of oxalate of potash; it precipitated with lime-water, a very divided powder, with sulphate of lime, in flakes, which would not readily unite; and with all the metallic solutions capable of precipitation by oxalic acid. Sulphate of alumine caused no precipitate, as it would have done with honistate of potash.

The potash found in the mother-water, after its precipitation by lime-water, and the disengagement of ammonia, caused by the addition of potash to the mother-water, prior to its decomposition by lime-water, sufficiently shew that these two alkalis saturate the acids contained in the mother-water of Guano;

The mother-water contains oxalates, phosphates, sulphates, and muriates of potash, and of ammonia.

Guano; and that the mother-water certainly contains oxalates, phofphates, fulphates, and muriates of potafh, and of ammonia.

The Guano left from the firft wafhing, —contains uric acid.

The five grammes and feven-tenths, left after the wafhing of the ten grammes originally taken for analyfis, were treated with cauftic potafh, which took up eight-tenths. This alkaline folution contained only uric acid, and a fmall portion of fat matter.

Phofphate of lime, iron, and carbonate of lime;

The 4.9 grammes left by the cauftic potafh, were treated with muriatic acid: the product was phofphate of lime, iron, and an atom of carbonate of lime.

—and left quartzofe and ferruginous fand.

After thefe applications of water, of cauftic potafh, and of muriatic acid, there remained of the 10 grammes of Guano, only 3.1 grammes of matter, compofed of quartzofe and ferruginous fands.

Recapitulation of component parts.

From the foregoing interefting analyfis, it appears that the manure of the iflets of the South Sea is formed of,—

1. Uric acid to the amount of $\frac{1}{4}$ of the whole compound; partly faturated with ammonia and lime:

2. Oxalic acid, partly faturated with ammonia and potafh:

3. Phofphoric acid, combined with the fame bafes and with lime:

4. Small quantities of fulphate and muriates of potafh and ammonia:

5. A fmall portion of fat matter:

6. Sand, partly quartzofe and partly ferruginous.

Remarks.

The exiftence of Guano in places frequented by vaft numbers of birds, and the identity of its nature with that of the excrements of aquatic birds, neceffarily throw confiderable light on the origin of this matter.

The analyfis proves how well founded was the ingenious comparifon of the learned naturalift, to whom we are indebted for our knowledge of this fubftance, no lefs interefting to us than ufeful to the inhabitants of Peru. It confirms the important difcovery made by the refearches of Meffrs. Fourcroy and Vauquelin. In a word, this analyfis poffeffes the advantage of proving a well-known maxim, that the fciences mutually enrich and enlighten each other with the light they poffefs; and it affords a new occafion to remark that among the fciences, there are perhaps none which have fo immediate and fo neceffary a connection as Chemiftry and Natural Hiftory.

XI.

Note on a Varnish for glazing Cups. By M. PARMENTIER*.

M. BOMPOIX, chief apothecary to the French Military Hospital at Genoa, having sent me some coffee-cups of a remarkable lightness, and glazed with a varnish which is held in great repute, perhaps only because its preparation is kept a secret in that country; I requested him to use his utmost endeavours to procure me the receipt. He obtained it through the medium of one of his pupils, who learned the secret from the artificer at the manufactory, and had made from his prescription a varnish in every respect equal to that in question: Account of very light coffee-cups finely varnished.

It consisted of lintseed oil 1 ½ lbs.; amber 1lb.; litharge in powder, minium in powder, ceruse in powder, each, 5 oz. Receipt for the varnish.

Boil the lintseed oil in an unglazed earthen vessel, and tie the litharge, minium, and ceruse in a linen bag, which is to be suspended in the oil whilst boiling, so that it may not touch the bottom of the vessel. When the oil begins to turn brown, take out the bag, and put in a clove of garlic, cleared of the skin; continue the boiling; and when the garlic is dried away, put in another and another, to the amount of six or seven. In the mean time, the amber is to be melted in another unglazed vessel, according to the method hereafter prescribed; and when the oil has been sufficiently boiled, the fused amber is to be poured into it.

To melt the Amber.

Take two ounces of lintseed oil, to soften the amber and to assist its fusion by a very brisk fire, and when the amber is melted, add the lintseed oil, and boil the whole about two minutes. The fluid must then be strained through a coarse cloth, and when cold put into a bottle well corked, to prevent from drying. Fusion of the amber.

Method of using the Varnish.

Let the piece intended to be varnished be first well polished, and then apply the varnish in the following manner: Application of the varnish.

* Annales de Chimie, Vol. LVI. p. 254.

Mix lamp black with varnifh and a little turpentine, with a hair pencil, and lay one coat on the piece; when this is dry, lay on another, and repeat the procefs till four coats have been laid on, taking care to let each dry before the application of the next. When the laft is dry, put the piece into a ftove or oven to complete the drying, and then polifh it with pumice and Tripoli powder.

Method of preparing the Piece intended to be varnifhed.

Manner of making the wooden cups.

Make the cups of hazel, alder, or cherry-tree, which are preferable to other woods for this ufe, becaufe they are porous when perfectly dry, and do not warp. Form them according to fancy, and dry them in an oven. The work muft be polifhed as if it were complete; and afterwards lay on the varnifh as already prefcribed.

Red varnifh.

If it fhould be wifhed to give a red ground to the article, mix a little minium, or rather cinabar, with the varnifh. Any other colour may in like manner be mixed with it, as may beft pleafe the fancy of the operator.

XII.

Account of a Series of Experiments, fhewing the Effects of Compreffion in modifying the Action of Heat. By* SIR JAMES HALL, *Bart. F. R. S. Edinburgh.*

SECTION I.

Ancient Revolutions of the Mineral Kingdom.—Vain Attempts to explain them.—Dependance of Geology on Chemiftry.—Importance of the Carbonate of Lime.—Dr BLACK'S *Difcovery of Carbonic Acid fubverted the former theories depending on Fire, but gave Birth to that of Dr.* HUTTON.—*Progrefs of the Author's Ideas with Regard to that Theory.—Experiments with Heat and Compreffion, fuggefted to Dr.* HUTTON *in* 1790.—*Undertaken by the Author in* 1798.—*Speculations on which his Hopes of Succefs were founded.*

Violent revolutions of the furface of the globe.

WHOEVER has attended to the ftructure of rocks and mountains, muft be convinced, that our globe has not always exifted

* The highly interefting experiments of Sir James Hall upon the effects of heat modified by compreffion, were communicated to the

-disturbed in its present state; but that every part of its mass, so far at least as our observations reach, has been agitated and subverted by the most violent revolutions.

Facts leading to such striking conclusions, however imperfectly observed, could not fail to awaken curiosity, and give rise to a desire of tracing the history, and of investigating the causes, of such stupendous events; and various attempts were made in this way, but with little success; for while discoveries of the utmost importance and accuracy were made in astronomy and natural philosophy, the systems produced by the Geologists were so fanciful and puerile, as scarcely to deserve a serious refutation. Geological systems imperfect.

One principal cause of this failure seems to have lain in the very imperfect state of chemistry, which has only of late years begun to deserve the name of a science. While chemistry was in its infancy, it was impossible that geology should make any progress; since several of the most important circumstances to be accounted for by this latter science, are admitted on all hands to depend upon principles of the former. The consolidation of loose sand into strata of solid rock; the crystalline arrangement of substances accompanying those strata, and blended with them in various modes, are circumstances of a because chemical knowledge was in its infancy.

the Royal Society of Edinburgh in August 1804, and were transmitted to our Journal by the author in the following month. They appear in Vol. IX. page 98. That concise narrative could not but strongly excite the curiosity of philosophers and geologists, and direct their earnest expectations to a fuller detail. In the last session, June 3, 1805, a very ample communication was made, which has been printed with five quarto plates, very beautifully engraved by Lizars, from designs by Sir James. I cannot but consider it as one of those high marks of approbation, with which the Philosophical Journal has been honoured from time to time, that the author has again directed his attention to this periodical work, as the vehicle through which his discoveries should be more extensively circulated. With this view he has not only favoured me with the memoir as soon as completed, but has liberally taken upon himself the expence of engraving the plates for the Journal in the same superior style. By this means the numbers containing his memoir will be enriched with ten additional plates besides those usually given :—for I shall with great satisfaction follow the steps of the worthy baronet by presenting the additional expences of paper and print to the reader without charge. W. N.

chemical

chemical nature, which all thofe who have attempted to frame theories of the earth have endeavoured by chemical reafonings to reconcile to their hypothefes.

Fire and water adduced as the agents in two theories.

Fire and *water*, the only agents in nature by which ftony fubftances are produced, under our obfervation, were employed by contending fects of geologifts, to explain all the phenomena of the mineral kingdom.

Water has little agency on minerals.

But the known properties of water are quite repugnant to the belief of its univerfal influence, fince a very great proportion of the fubftances under confideration are infoluble, or nearly fo, in that fluid; and fince, if they were all extremely foluble, the quantity of water which is known to exift, or that could poffibly exift in our planet, would be far too fmall to accomplifh the office affigned to it in the Neptunian theory *. On

Common fire does not explain the facts.

the other hand, the known properties of fire are no lefs inadequate to the purpofe; for, various fubftances which frequently occur in the mineral kingdom, feem, by their prefence, to preclude its fuppofed agency; fince experiment fhews, that, in our fires, they are totally changed or deftroyed.

Hence both theories were doubtful.

Under fuch circumftances, the advocates of either element were enabled, very fuccefsfully, to refute the opinions of their adverfaries, though they could but feebly defend their own: and, owing, perhaps to this mutual power of attack, and for want of any alternative to which the opinions of men could lean, both fyftems maintained a certain degree of credit; and writers on geology indulged themfelves, with a fort of impunity, in a ftyle of unphilofophical reafoning, which would not have been tolerated in other fciences.

Carbonate of lime is of extenfive importance,

Of all mineral fubftances, the *carbonate of lime* is unqueftionably the moft important in a general view. As limeftone or marble, it conftitutes a very confiderable part of the folid mafs of many countries; and, in the form of veins and nodules of fpar, pervades every fpecies of ftone. Its hiftory is thus interwoven in fuch a manner with that of the mineral kingdom at large, that the fate of any geological theory muft very much depend upon its fuccefsful application to the various conditions of this fubftance. But, till Dr. Black, by his

* *Illuftrations of the Huttonian Theory*, by Mr. Profeffor Playfair, § 430.

difcovery

discovery of carbonic acid, explained the chemical nature of the carbonate, no rational theory could be formed, of the chemical revolutions which it has undoubtedly undergone.

This discovery was, in the first instance, hostile to the supposed action of fire; for the decomposition of limestone by fire in every common kiln being thus proved, it seemed absurd to ascribe to that same agent the formation of limestone, or of any mass containing it.

seems not producible by heat.

The contemplation of this difficulty led Dr. Hutton to view the action of fire in a manner peculiar to himself, and thus to form a geological theory, by which, in my opinion, he has furnished the world with the true solution of one of the most interesting problems that has ever engaged the attention of men of science.

Dr. Hutton's theory.

He supposed,

I. That heat has acted, at some remote period, on all rocks.

That rocks have undergone heat under strong pressure.

II. That during the action of heat, all these rocks (even such as now appear at the surface) lay covered by a superincumbent mass, of great weight and strength.

III. That in consequence of the combined action of heat and pressure, effects were produced different from those of heat on common occasions; in particular, that the carbonate of lime was reduced to a state of fusion, more or less complete, without any calcination.

The essential and characteristic principle of his theory is thus comprised in the word *compression;* and by one bold hypothesis, founded on this principle, he undertook to meet all the objections to the action of fire, and to account for those circumstances in which minerals are found to differ from the usual products of our furnaces.

This system, however, involves so many suppositions, apparently in contradiction to common experience, which meet us on the very threshold, that most men have hitherto been deterred from the investigation of its principles, and only a few individuals have justly appreciated its merits. It was long before I belonged to the latter class; for I must own, that, on reading Dr. Hutton's first geological publication, I was induced to reject his system entirely, and should probably have continued still to do so, with the great majority of the world, but for my habits of intimacy with the author; the vivacity

Singular contrast of the perspicuity of Dr. Hutton's conversation, and the obscurity of his writings.

and

and perſpicuity of whoſe converſation formed a ſtriking contraſt to the obſcurity of his writings. I was induced by that charm, and by the numerous original facts which his ſyſtem had led him to obſerve, to liſten to his arguments, in favour of opinions which I then looked upon as viſionary. I thus derived from his converſation the ſame advantage which the world has lately done from the publication of Mr. Playfair's *Illuſtrations;* and, experienced the ſame influence which is now exerted by that work, on the minds of our moſt eminent men of ſcience.

The author' progreſs in the Doctor's theory.

After three years of almoſt daily warfare with Dr. Hutton, on the ſubject of his theory, I began to view his fundamental principles with leſs and leſs repugnance. There is a period, I believe, in all ſcientific inveſtigations, when the conjectures of genius ceaſe to appear extravagant; and when we balance the fertility of a principle, in explaining the phenomena of nature, againſt its improbability as an hypotheſis: The partial view which we then obtain of truth, is perhaps the moſt attractive of any, and moſt powerfully ſtimulates the exertions of an active mind. The miſt which obſcured ſome objects diſſipates by degrees, and allows them to appear in their true colours; at the ſame time, a diſtant proſpect opens to our view, of ſcenes unſuſpected before.

He propoſes experimental confirmation,

Entering now ſeriouſly into the train of reaſoning followed by Dr. Hutton, I conceived that the chemical effects aſcribed by him to compreſſion, ought, in the firſt place, to be inveſtigated; for, unleſs ſome good reaſon were given us for believing that heat would be modified by preſſure, in the manner alledged, it would avail us little to know that they had acted together. He reſted his belief of this influence on analogy; and on the ſatisfactory ſolution of all the phenomena furniſhed by this ſuppoſition. It occurred to me, however, that this principle was ſuſceptible of being eſtabliſhed in a direct manner by experiment, and I urged him to make the attempt; but he always rejected this propoſal, on account of the immenſity of the natural agents, whoſe operations he ſuppoſed to lie far beyond the reach of our imitation; and he ſeemed to imagine, that any ſuch attempt muſt undoubtedly fail, and thus throw diſcredit on opinions already ſufficiently eſtabliſhed, as he conceived, on other principles. I was far, however, from

rejected by the Doctor.

from being convinced by these arguments; for, without being able to prove that any artificial compression to which we could expose the carbonate, would effectually prevent its calcination in our fires, I maintained, that we had as little proof of the contrary, and that the application of a moderate force might possibly perform all that was hypothetically assumed in the Huttonian theory. On the other hand, I considered myself as bound, in practice, to pay deference to his opinion, in a field which he had already so nobly occupied; and abstained, during the remainder of his life, from the prosecution of some experiments with compression, which I had begun in 1790.

In 1798, I resumed the subject with eagerness, being still of opinion that the chemical law which forms the basis of the Huttonian theory, ought, in the first place, to be investigated experimentally; all my subsequent reflections and observations having tended to confirm my idea of the importance of this pursuit, without in any degree rendering me more apprehensive as to the result. Experimental investigation undertaken.

In the arrangement of the following paper, I shall first confine myself to the investigation of the chemical effects of heat and compression, reserving to the concluding part the application of my results to Geology. I shall then appeal to the volcanoes, and shall endeavour to vindicate the laws of action assumed in the Huttonian theory, by shewing, that lavas, previous to their eruptions, are subject to similar laws; and that the volcanoes, by their subterranean and submarine exertions, must produce, in our times, results similar to those ascribed, in that theory, to the former action of fire. Order of the present treatise.

In comparing the Huttonian operations with those of the volcanoes, I shall avail myself of some facts, brought to light in the course of the following investigations, by which a precise limit is assigned to the intensity of the heat, and to the force of compression, required to fulfil the conditions of Dr. Hutton's hypothesis: For, according to him, the power of those agents was very great, but quite indefinite; it was therefore impossible to compare their supposed effects in any precise manner with the phenomena of nature.

My attention was almost exclusively confined to the carbonate of lime, about which I reasoned as follows: The carbonic acid, when uncombined with any other substance, exists natu- Argument respecting carbonate of lime.

 rally

rally in a gaseous form, at the common temperature of our atmosphere; but when in union with lime, its volatility is repressed, in that same temperature, by the chemical force of the earthy substance, which retains it in a solid form. When the temperature is raised to a full red-heat, the acid acquires a volatility by which that force is overcome, it escapes from the lime, and assumes its gaseous form. It is evident, that were the attractive force of the lime increased, or the volatility of the acid diminished by any means, the compound would be enabled to bear a higher heat without decomposition, than it can in the present state of things. Now, pressure must produce an effect of this kind; for when a mechanical force opposes the expansion of the acid, its volatility must, to a certain degree, be diminished. Under pressure, then, the carbonate may be expected to remain unchanged in a heat, by which, in the open air, it would have been calcined. But experiment alone can teach us what compressing force is requisite to enable it to resist any given elevation of temperature; and what is to be the result of such an operation. Some of the compounds of lime with acids are fusible, others refractory; the carbonate, when constrained by pressure to endure a proper heat, may be as fusible as the muriate.

Pressure must oppose the expansion and escape of the acid, and enable it to support a stronger heat.

Probability that the carbonate might not be of difficult fusion.

One circumstance, derived from the Huttonian Theory, induced me to hope, that the carbonate was easily fusible, and indicated a precise point, under which that fusion ought to be expected. Nothing is more common than to meet with nodules of calcareous spar inclosed in whinstone; and we suppose, according to the Huttonian theory, that the whin and the spar had been liquid together; the two fluids keeping separate, like oil and water. It is natural, at the junction of these two, to look for indications of their relative fusibilities; and we find, accordingly, that the termination of the spar is generally globular and smooth; which seems to prove, that, when the whin became solid, the spar was still in a liquid state; for had the spar congealed first, the tendency which it shews, on all occasions of freedom, to shoot out into prominent crystals, would have made it dart into the liquid whin, according to the peculiar forms of its crystallization; as has happened with the various substances contained in whin, much more refractory than itself, namely, augite, felspar, &c.; all of which having congealed in the liquid whin, have assumed their peculiar

Facts which indicate its melting heat.

forms

forms with perfect regularity. From this I concluded, that when the whin congealed, which must have happened about 28° or 30° of Wedgwood, the spar was still liquid. I therefore expected, if I could compel the carbonate to bear a heat of 28° without decomposition, that it would enter into fusion. The sequel will shew that this conjecture was not without foundation.

I shall now enter upon the description of those experiments, the result of which I had the honour to lay before this Society on the 30th of August last (1804); fully aware how difficult it is, in giving an account of above five hundred experiments, all tending to one point, but differing much from each other in various particulars, to steer between the opposite faults of prolixity and barrenness. My object shall be to describe, as shortly as possible, all the methods followed, so as to enable any chemist to repeat the experiments; and to dwell particularly on such circumstances only as seem to lead to conclusions of importance. The experiments introduced.

The result being already known, I consider the account I am about to give of the execution of these experiments, as addressed to those who take a particular interest in the progress of chemical operations: in the eyes of such gentlemen, I trust, that none of the details into which I must enter, will appear superfluous.

Section II.

Principle of Execution upon which the following Experiments were conducted.—Experiments with Gun-Barrels filled with baked Clay, and welded at the Muzzle.—Method with the fusible Metal.—Remarkable Effects of its Expansion.—Necessity of introducing Air.—Results obtained.

When I first undertook to make experiments with heat acting under compression, I employed myself in contriving various devices of screws, of bolts, and of lids, so adjusted, I hoped, as to confine all elastic substances; and perhaps some of them might have answered. But I laid aside all such devices, in favour of one which occurred to me in January 1798; which, by its simplicity, was of easy application in all cases, and accomplished all that could be done by any device, since it secured perfect strength and tightness to the utmost that the vessels employed could bear, whether formed of metallic or earthy substance. The device depends upon the following general view: If we take a hollow tube or barrel (AD *Pl.* ix. fig.

The author's early contrivances for confining elastic substances at high temperatures.

The method adopted ultimately was to include the subject in an iron barrel, and close the aperture by fusion;

fig. 1.)* closed at one end, and open at the other, of one foot or more in length; it is evident, that by introducing one end into a furnace, we can apply to it as great heat as art can produce, while the other end is kept cool, or, if necessary, exposed to extreme cold. If, then, the substance which we mean to subject to the combined action of heat and pressure be introduced into the breech or closed end of the barrel (CD), and if the middle part be filled with some refractory substance, leaving a small empty space at the muzzle (AB), we can apply heat to the muzzle, while the breech containing the subject of experiment, is kept cool, and thus close the barrel by any of the numerous modes which heat affords, from the welding of iron to the melting of sealing-wax. Things being then reversed, and the breech put into the furnace, a heat of any required intensity may be applied to the subject of experiment, now in a state of constraint.

First experiment with the muzzle plugged and welded,

My first application of this scheme was carried on with a common gun-barrel, cut off at the touch-hole, and welded very strongly at the breech by means of a plug of iron. Into it I introduced the carbonate, previously rammed into a cartridge of paper or pasteboard, in order to protect it from the iron, by which, in some former trials, the subject of experiment had been contaminated throughout during the action of heat. I then rammed the rest of the barrel full of pounded clay previously baked in a strong heat, and I had the muzzle closed like the breech, by a plug of iron welded upon it in a common forge; the rest of the barrel being kept cold during this operation, by means of wet cloths. The breech of the barrel was then put horizontally into a common muffle, heated to about 25° of Wedgwood. To the muzzle a rope was fixed, in such a manner, that the barrel could be withdrawn without danger from an explosion †. I likewise, about this time closed the muzzle of

in another instance soldered.

* This plate will be given in No. 54, being the supplement to the present volume.

† On one occasion, the importance of this precaution was strongly felt. Having inadvertently introduced a considerable quantity of moisture into a welded barrel, an explosion took place, before the heat had risen to redness, by which, part of the barrel was spread out to a flat plate, and the furnace was blown to pieces. Dr. Kennedy, who happened to be present on this occasion, observed, that notwithstanding this accident, the time might come when we should employ water in these experiments to assist the force

of the barrel, by means of a plug, fixed by folder only; which method had this peculiar advantage, that I could fhut and open the barrel without having recourfe to a workman. In thefe trials, though many barrels yielded to the expanfive force, others refifted it, and afforded fome refults that were in the higheft degree encouraging, and even fatisfactory, could they have been obtained with certainty on repetition of the procefs. In many of them, chalk, or common limeftone previoufly pulverifed, was agglutinated into a ftony mafs, which required a fmart blow of a hammer to break it, and felt under the knife like a common limeftone; at the fame time, the fubftance, when thrown into nitric acid, diffolved entirely with violent effervefcence.

Satisfactory refults.

In one of thefe experiments, owing to the action of heat on the cartridge of paper, the baked clay, which had been ufed to fill the barrel, was ftained black throughout, to the diftance of two-thirds of the length of the barrel from its breech. This circumftance is of importance, by fhewing, that though all is tight at the muzzle, a protrufion may take place along the barrel, greatly to the detriment of complete compreffion: and, at the fame time, it illuftrates what has happened occafionally in nature, where the bituminous matter feems to have been driven by fuperior local heat, from one part of a coaly bed, though retained in others, under the fame compreffion. The bitumen fo driven off being found, in other cafes, to pervade and tinge beds of flate and of fandftone.

Volatile matter may be driven from one to another part of a clofed barrel.

I was employed in this purfuit in fpring 1800, when an event of importance interrupted my experiments for about a year. But I refumed them in March 1801, with many new plans of execution, and with confiderable addition to my apparatus.

In the courfe of my firft trials, the following mode of execution had occurred to me, which I now began to put in practice. It is well known to chemifts, that a certain compofition of

Experiments in which the fufible metal was ufed as the plug.

force of compreffion. I have fince made great ufe of this valuable fuggeftion: but he fcarcely lived, alas! to fee its application; for my firft fuccefs in this way took place during his laft illnefs.—I have been expofed to no rifk in any other experiment with iron barrels; matters being fo arranged, that the ftrain againft them has only commenced in a red heat, in which the metal has been fo far foftened, as to yield by laceration like a piece of leather.

different

different metals*, produces a fubftance fo fufible, as to melt in the heat of boiling-water. I conceived that great advantage, both in point of accuracy and difpatch, might be gained in thefe experiments, by fubftituting this metal for the baked clay above mentioned: That after introducing the carbonate into the breech of the barrel, the fufible metal, in a liquid ftate, might be poured in, fo as to fill the barrel to its brim: That when the metal had cooled and become folid, the breech might, as before, be introduced into a muffle, and expofed to any required heat, while the muzzle was carefully kept cold. In this manner, no part of the fufible metal being melted but what lay at the breech, the reft, continuing in a folid ftate, would effectually confine the carbonic acid: That after the action of ftrong heat had ceafed, and after all had been allowed to cool completely, the fufible metal might be removed entirely from the barrel, by means of a heat little above that of boiling water, and far too low to occafion any decompofition of the carbonate by calcination, though acting upon it in freedom; and then, that the fubject of experiment might, as before, be taken out of the barrel.

Advantages of this method.

This fcheme, with various modifications and additions, which practice has fuggefted, forms the bafis of moft of the following methods.

A ftriking phenomenon. When the barrel was completely filled with fufible metal only, and the clofed end of the iron expofed to heat in a muffle, the greater expanfion of the fluid forced it through the texture of the iron in very fine wire refembling wool.

In the firft trial, a ftriking phenomenon occurred, which gave rife to the moft important of thefe modifications. Having filled a gun-barrel with the fufible metal, without any carbonate; and having placed the breech in a muffle, I was furprifed to fee, as the heat approached to rednefs, the liquid metal exuding through the iron in innumerable minute drops, difperfed all round the barrel. As the heat advanced, this exudation increafed, till at laft the metal flowed out in continued ftreams, and the barrel was quite deftroyed. On feveral occafions of the fame kind, the fufible metal, being forced through fome very minute aperture in the barrel, fpouted from it to the diftance of feveral yards, depofiting upon any fubftance oppofed to the ftream, a beautiful affemblage of fine wire, exactly in the form of wool. I immediately underftood that the phenomenon was produced by the fuperior expanfion of the liquid over the folid metal, in con-

* Eight parts of bifmuth, five of lead, and three of tin.

feouence

ſequence of which, the fuſible metal was driven through the iron as water was driven through ſilver * by mechanical percuſſion in the Florentine experiment. It occurred to me, that this might be prevented by confining along with the fuſible metal a ſmall quantity of air, which, by yielding a little to the expanſion of the liquid, would ſave the barrel. This remedy was found to anſwer completely, and was applied, in all the experiments made at this time †.

Remedy. A ſmall quantity of air was left in the barrel.

I now propoſed, in order to keep the carbonate clean, to incloſe it in a ſmall veſſel; and to obviate the difficulty of removing the reſult at the concluſion of the experiment, I further propoſed to connect that veſſel with an iron ramrod, longer than the barrel, by which it could be introduced or withdrawn at pleaſure.

The carbonate was incluted in a ſmall ſeparate veſſel.

* *Eſſays of Natural Experiments made in the Academie del Cimento*, tranſlated by Waller, London, 1684, page 117. The ſame in Muſſchenbroek's Latin Tranſlation, Ludg. Bat. 1731, p. 63.

† I found it a matter of much difficulty to aſcertain the proper quantity of air which ought to be thus incloſed. When the quantity was too great, the reſult was injured by diminution of elaſticity, as I ſhall have occaſion fully to ſhew hereafter. When too ſmall, or when, by any accident, the whole of this included air was allowed to eſcape, the barrel was deſtroyed.

I hoped to aſcertain the bulk of air neceſſary to give liberty to the expanſion of the liquid metal, by meaſuring the actual quantity expelled by known heats from an open barrel filled with it. But I was ſurpriſed to find, that the quantity thus diſcharged, exceeded in bulk that of the air which, in the ſame heats, I had confined along with the carbonate and fuſible metal in many ſucceſsful experiments. As the expanſion of the liquid does not ſeem capable of ſenſible diminution by an oppoſing force, this fact can only be accounted for by a diſtenſion of the barrel. In theſe experiments, then, the expanſive force of the carbonic acid, of the included air, and of the fuſible metal, acted in combination againſt the barrel, and were yielded to in part by the diſtenſion of the barrel, and by the condenſation of the included air. My object was to increaſe the force of this mutual action, by diminiſhing the quantity of air, and by other devices to be mentioned hereafter. Where ſo many forces were concerned, the laws of whoſe variations were unknown, much preciſion could not be expected, nor is it wonderful, that in attempting to carry the compreſſing force to the utmoſt, I ſhould have deſtroyed barrels innumerable.

A ſmall

Description of this apparatus

A ſmall tube of glaſs,* or of Reaumur's porcelain, about a quarter of an inch in diameter, and one or two inches in length, (fig. 2, A) was half filled with pounded carbonate of lime, rammed as hard as poſſible; the other half of the tube being filled with pounded ſilex, or with whatever occurred as moſt likely to prevent the intruſion of the fuſible metal in its liquid and penetrating ſtate. This tube ſo filled, was placed in a frame or cradle of iron (*d f k h*, figs. 3, 4, 5, and 6), fixed to the end (*m*) of a ram-rod (*m n*). The cradle was from ſix to three inches in length, and as much in diameter as a gun-barrel would admit with eaſe. It was compoſed of two circular plates of iron, (*d e f g* and *h i k l*, ſeen edgewiſe in the figures), placed at right-angles to the ram-rod, one of theſe plates (*d e f g*) being fixed to it by the centre (*m*). Theſe plates were connected together by four ribs or flattened wires of iron (*d h*, *e i*, *f k*, and *g l*,) which formed the cradle into which the tube (A), containing the carbonate, was introduced by thruſting the adjacent ribs aſunder. Along with the tube juſt mentioned, was introduced another tube (B), of iron or porcelain, filled only with air. Likewiſe, in the cradle, a pyrometer † piece (C) was placed in contact with (A) the tube con-

* I have ſince conſtantly uſed tubes of common porcelain, finding glaſs much too fuſible for this purpoſe.

† The pyrometer-pieces uſed in theſe experiments were made, under my own eye. Neceſſity compelled me to undertake this laborious and difficult work, in which I have already ſo far ſucceeded as to obtain a ſet of pieces, which, though far from complete, anſwer my purpoſe tolerably well. I had lately an opportunity of comparing my ſet with that of Mr. Wedgwood, at various temperatures, in furnaces of great ſize and ſteadineſs. The reſult has proved, that my pieces agree as well with each other as his, though with my ſet each temperature is indicated by a different degree of the ſcale. I have thus been enabled to conſtruct a table, by which my obſervations have been corrected, ſo that the temperatures mentioned in this paper are ſuch as would have been indicated by Mr. Wedgwood's pieces. By Mr. Wedgwood's pieces, I mean thoſe of the only ſet which has been ſold to the public, and by which the melting heat of pure ſilver is indicated at the 22d degree. I am well aware, that the late Mr. Wedgwood, in his Table of Fuſibilities, has ſtated that fuſion as taking place

containing the carbonate. Theſe articles generally occupied the whole cradle; when any ſpace remained, it was filled up by a piece of chalk dreſſed for the purpoſe. (Fig. 4, repreſents the cradle filled, as juſt deſcribed).

Method of uſing the ſame,

Things being thus prepared, the gun-barrel, placed erect with its muzzle upwards, was half filled with the liquid fuſible metal. The cradle was then introduced into the barrel, and plunged to the bottom of the liquid, ſo that the carbonate was placed very near the breech, (as repreſented in fig. 5, the fuſible metal ſtanding at *o*). The air-tube (B) being placed ſo as to enter the liquid with its muzzle downwards, retained great part of the air it originally contained, though ſome of it might be driven off by the heat, ſo as to eſcape through the liquid. The metal being now allowed to cool, and to fix round the cradle and ramrod, the air remaining in the air-tube was effectually confined, and all was held faſt. The barrel being then filled to the brim with fuſible metal, the apparatus was ready for the application of heat to the breech, (as ſhewn in fig. 6.) Plate X.

the furnace and muffle arrangement, &c.

In the experiments made at this time, I uſed a ſquare brick furnace, (figs. 7 and 8, having a muffle (*r s*) traverſing it horizontally and open at both ends. This muffle being ſupported in the middle by a very ſlender prop, was expoſed to fire from below, as well as all round. The barrel was placed in the muffle, with its breech in the hotteſt part, and the end next the muzzle projecting beyond the furnace, and ſurrounded with cloths which were drenched with water from time to time. (This arrangement is ſhewn in fig. 7.) In this ſituation, the fuſible metal ſurrounding the cradle being melted, the air contained in the air-tube would of courſe ſeek the higheſt poſition, and its firſt place in the air-tube would be occupied by fuſible metal. (In fig. 6, the new poſition of the air is ſhewn at *p q*).

Method of diſengaging the contents after experiment.

At the concluſion of the experiment, the metal was generally removed by placing the barrel in the tranſverſe muffle, with its muzzle pointing a little downwards, and ſo that the heat was applied firſt to the muzzle, and then to the reſt of the barrel in ſucceſſion. (This operation is ſhewn in fig. 8.) In

place at the 28th degree; but I am convinced that his obſervations muſt have been made with ſome ſet different from that which was afterwards ſold.

ſome of the firſt of theſe experiments, I looſened the cradle, by plunging the barrel into heated brine, or a ſtrong ſolution of muriate of lime; which laſt bears a temperature of 250° of Fahrenheit before it boils. For this purpoſe, I uſed a pan three inches in diameter, and three feet deep, having a flat baſon at top to receive the liquid when it boiled over. The method anſwered, but was troubleſome, and I laid it aſide. I have had occaſion, lately, however, to reſume it in ſome experiments in which it was of conſequence to open the barrel with the leaſt poſſible heat*.

By theſe methods I made a great number of experiments, with reſults that were highly intereſting in that ſtage of the buſineſs, though their importance is ſo much diminiſhed by the ſubſequent progreſs of the inveſtigation, that I think it proper to mention but very few of them.

Calcareous ſpar converted into hard denſe marble by heat of 33° Wedgwood.

On the 31ſt of March, 1801, I rammed forty grains of pounded chalk into a tube of green bottle-glaſs, and placed it in the cradle as above deſcribed. A pyrometer in the muffle along with the barrel indicated 33°. The barrel was expoſed to heat during ſeventeen or eighteen minutes. On withdrawing the cradle, the carbonate was found in one ſolid maſs, which had viſibly ſhrunk in bulk, the ſpace thus left within the tube being accurately filled with metal, which plated the carbonate all over without penetrating it in the leaſt, ſo that the metal was eaſily removed. The weight was reduced from forty to thirty-ſix grains. The ſubſtance was very hard, and reſiſted the knife better than any reſult of the kind previouſly obtained; its fracture was cryſtalline, bearing a reſemblance to white ſaline marble; and its thin edges had a decided ſemitranſparency, a circumſtance firſt obſerved in this reſult.

Calcareous ſpar rendered cryſtalline with conchoidal fracture by heat 23°.

On the 3d of March of the ſame year, I made a ſimilar experiment, in which a pyrometer-piece was placed within

* In many of the following experiments, lead was uſed in place of the fuſible metal, and often with ſucceſs; but I loſt many good reſults in this way: for the heat required to liquefy the lead approaches ſo near to redneſs, that it is difficult to diſengage the cradle without applying a temperature by which the carbonate is injured. I have found it anſwer well, to ſurround the cradle and a few inches of the tube with fuſible metal, and to fill the reſt of the barrel with lead.

the barrel, and another in the muffle; they agreed in indicating 23°. The inner tube, which was of Reaumur's porcelain, contained eighty grains of pounded chalk. The carbonate was found, after the experiment, to have lost $3\frac{1}{2}$ grains. A thin rim, less than the 20th of an inch in thickness, of whitish matter, appeared on the outside of the mass. In other respects, the carbonate was in a very perfect state; it was of a yellowish colour, and had a decided semitransparency and saline fracture. But what renders this result of the greatest value, is, that on breaking the mass, a space of more than the tenth of an inch square, was found to be completely crystallized, having acquired the rhomboidal fracture of calcareous spar. It was white and opaque, and presented to the view three sets of parallel plates which are seen under three different angles. This substance, owing to partial calcination and subsequent absorption of moisture, had lost all appearance of its remarkable properties in some weeks after its production; but this appearance has since been restored, by a fresh fracture, and the specimen is now well preserved by being hermetically inclosed,

(To be continued.)

XIII.

On the Use of the Sutures in the Skulls of Animals. By Mr. B. Gibson *.

THE full use of the singular junction of the bones of the skull, which is called suture, has, from the earliest periods of anatomy and surgery, attracted the attention and eluded the researches of the physiologist. To this remarkable feature in osteogny, in a great measure peculiar to a certain period of life, many uses have been attributed. Some of these are totally erroneous; such as that for allowing the transpiration of moisture, to keep the brain cool and fit for thinking; for giving a more strict adhesion of the dura mater to the inner surface of the skull; for admitting a more free communication by blood-vessels between the external and internal parts of the head; or for affording interstices, that the bones may be pushed

Conjectures on the use of the sutures in the skulls of animals.

* Manchester Memoirs, N. S. Vol. I. 39.

pufhed afunder by the growth of the brain, left that organ fhould be cramped in its growth, in confequence of the comparatively flow growth of the bones of the fkull.

Other fuppofed ufes.

Other ufes attributed to the futures are merely flight advantages derived from their ftructure, which are enjoyed in early infancy, or till adult life, but gradually ceafe after that period. Thus at the time of birth the loofe union of the bones of the fkull accommodates the fhape of the head to the figure of the different parts of the cavity through which it paffes. At adult age, when the futures are fully formed, they may occafionally check the progrefs (if I may be allowed the expreffion) of a fracture nearly fpent;—or vibrations, communicated to the bones of the fkull, will be propagated with lefs force to the brain, in confequence of the bones being feparated at the futures. It is, however, abundantly evident, that thefe are not the main purpofes for which the futures are formed; otherwife they would not begin to be obliterated at a period of life when they would perform thefe offices more ufefully than ever. Confiftent with this remark we fhall find, that the true purpofe for which they are formed, and the particular procefs with which they are connected, is fully completed before their obliteration takes place.

The cartilage between bones deftined to be united, difappears at laft.

When we take a view of the mode of junction between many bones, and parts of bones in the human body, which do not admit of motion, we find that with little exception they all agree in this particular; that fooner or later the cartilage or periofteum which once was interpofed is obliterated, and thefe different portions, or entire bones, coalefce.

Inftances [illegible] the [illegible] and [illegible] bones.

The feparate portions, which originally compofe the vertebræ, are early in thus uniting: after thefe the fides of the lower jaw; at a later period the epiphyfis of a cylindrical bone is united to its body: and ftill later the bones of the fkull ufually coalefce, and the futures are obliterated. Other bones, as thofe of the face, which have no motion and fuftain little weight, are irregular in this refpect; fometimes uniting, but generally remaining diftinct, to the end of a long life.

Manner in which the offeous fyftem is completed, &c.

The original formation of the offeous fyftem in feveral diftinct pieces, refpects principally its fpeedy offification at an early period of life, and its future convenient extenfion, till it has arrived at its full growth; and we may confider it as

a general principle, that where two parts of *one* bone are separated from each other by an intervening cartilage, or *two* distinct bones merely by periosteum, at that part osseous materials are added to increase their length or extend their superfices. This we shall find takes place, whether the junction be effected by comparatively smooth surfaces, as between the body of a bone and its epiphysis; or between the bones of the skull by jagged sutures. Hence it appears that the bones of the body generally are increased in length or extent, not by a uniform extension of the whole substance, but by an addition of bony matter in some particular part.

Thus the body of a cylindrical bone is lengthened by addition to each end. This we might conclude would be the case, from considering the part in which its ossification commences: as this commences in a middle point and proceeds to each extremity, it is natural to suppose that its growth still goes on in the same direction, or continues at the extremities. That this is the case we know, not by reasoning alone, but by a direct experiment. Mr. Hunter sunk two small pieces of lead in the middle of the tibia, or shin bone of a pig, and measured accurately the distance between them; on examining the animal some time afterwards, it appeared, that though the bone had increased considerably in length, the pieces of lead still remained at the same distance from each other that they were before. From this experiment we learn, that a cylindrical bone is not extended in its middle, but is lengthened by addition to its extremities, where the body of the bone is joined to its epiphysis; the chief intention of the epiphysis being to allow the intervention of a vascular organ, which may conveniently deposit bony materials, without interfering with the joint itself.

Cylindrical bones are lengthened by additions at each end.

As cylindrical bones are lengthened at their extreme parts, we are led by analogy to conclude, that the same general plan is pursued in the extension of the flat bones of the body: and although we have no direct experiment by which this has been proved, there are circumstances which leave little doubt but they are extended by addition to their edges. Thus to take the parietal bone as an example; as ossification begins in a central point and extends towards the circumference, it is probable that to the completion of the process, it continues to go on in the same direction; and the same circumstance taking

The same process appears to take place at the edges of flat bones.

plac-

place in every bone of the cranium, it is probable that even after the whole of the brain is incaſed in bone, the addition is ſtill made at the edge of each, and that the general enlargement originates where they are all mutually joined by the ſutures. Of this proceſs I had a very ſtriking illuſtration ſome years ago. In a young ſubject, from what cauſe I know not, the depoſition of oſſeous matter had been ſuddenly increaſed a ſhort time before death. It was in different ſtages of progreſs, but had taken place in all the bones of the body which I preſerved; in ſome partially, in others generally. In all, the new oſſeous matter was elevated above the level of the bone upon which it was placed. In ſome parts of the parietal bones it was only in its commencement, and put on the appearance of a net-work, ſimilar to that which may be obſerved in the ſame bones at an early period of their formation. In other parts the meſhes of the net-work were more or leſs filled up; in others again completely, ſo as to put on the uniform appearance of ſolid bone. The ſame reticulated appearance was evident on the edges of all the bones of the ſkull, where they form the ſutures, and at the extremities of the cylindrical bones, between the body and epiphyſis. The ſame appearance of increaſed depoſition was ſeen on the ſurface of the cylindrical bones, with this difference, that the meſhes were not circular, but oblong ſquares; ſo as to put on more of the ſtriated appearance. In ſome parts, the newly ſecreted bone was eaſily ſeparable from the general maſs, and formed a thin layer externally, affording one of the beſt proofs I have met with, of the increaſe of cylindrical bones in thickneſs by depoſition externally, whilſt a correſponding internal abſorption goes on. From the ſtriking ſimilarity of appearance on the ſurfaces and edges of the bones, we may ſafely conclude, that the ſame proceſs of depoſition was going on in both, and may thence infer, that the bones of the ſkull are increaſed in extent by the depoſition of oſſeous matter at their edges, or where they are joined to each other by ſuture. This fact points out to us, in a great meaſure, the real uſe of this peculiar mode of junction.

Inſtance in the ſkull.

In order that the bones of the ſkull may be increaſed in extent, it is neceſſary that they ſhould be retained at a certain diſtance from each other; that the perioſteum with its veſſels

The ſerrated edges give firmneſs, &c.

2 may

may paſs down between them, free from compreſſion and ſecrete the oſſeous matter. At the ſame time, the thin bones compoſing the upper part of the ſkull, reſting as an arch upon its baſis, muſt be united together ſo firmly, as not to be ſeparated by common degrees of violence. For this purpoſe, projecting points from the external ſurface of each bone, are reciprocally received into correſponding nitches; which only penetrate through one half of the thickneſs of the ſkull, and form an irregular kind of dovetailing.

Two advantages ariſe from this ſtructure, being ſuperficial, and confined to the external table of the ſkull. The projecting points from each ſide, reſting upon the ſolid ſurface of the internal table of the oppoſite bone, can reſiſt more effectually any violence, which might tend to force the bones inwards; and the internal part of the ſkull preſents, by this means, a ſmooth ſurface to the coverings of the brain; for internally no appearance of a jagged ſuture is ſeen.

From this view of the ſubject we ſee, that the ſutures of the human ſkull, by their peculiar formation, at once unite the bones together, and ſo far ſeparate them, as to allow the interpoſition of a vaſcular organ by which their ſuperficies is gradually increaſed to its greateſt extent *. This explanation of

Thus the ſutures unite the bones, and admit the vaſcular organ requiſite for their growth.

* Since this paper was written in the year 1800, I have found, that a ſimilar opinion was publiſhed by Profeſſor Soemmerring in 1794, in his valuable work, "De corporis humani fabrica." To him, therefore, any credit which may belong to the primary ſuggeſtion of this uſe of the ſutures is due. As his opinion, however, has been little noticed by anatomiſts generally, and is placed in a clearer point of view by the facts which ſuggeſted this further explanation of it to me, it has not been thought improper to give this eſſay a place in theſe Memoirs. But whilſt the reader will ſee, by the following quotation, the near reſemblance between the opinion of Profeſſor Soemmerring and that which I have brought forward, I hope the character of plagiariſt or compiler will not be attributed to me.

"Uſu horum ſic ſeſe habentium terminorum oſſa cranii inter bene liquet.

"Incrementum ambitus calvariæ levant, ni enim inter oſſa capitis mox poſt partum ſuturæ interponerentur, hæc creſcere non poſſent, niſi aliâ ratione natura rem inſtitueret. Tali igitur modo incrementum calvariæ cum incremento reliquorum oſſium convenit; initio enim ſuturis, vel potius lineis cartilaginoſis oſſa iis

of the use of sutures comprehends and accounts for those concomitant circumstances, which were considered by older anatomists as their real use; and, as far as I can see, is not contradicted by any fact connected with them.

Other remark and inferences

If it be asked, for instance, why at the sutures there is a stronger adhesion of the *dura mater* internally and *periosteum* externally than in other parts of the skull? the answer is, that these membranes with their vessels are continued into the sutures, to form conjointly the secretory organ, by which the bones are extended.

If it be asked, why there is a greater vascularity or an appearance of blood-vessels passing through the sutures? it is perfectly consistent with this opinion to answer, that the increase of blood goes to this secretory organ, for the purpose of the extension of the bones.

Why the sutures are obliterated, &c.

The explanation here offered accounts also for the general obliteration of the sutures after a certain period of life; for the bones having then arrived at their full size, the organ for the secretion of osseous matter is no longer needed; it shrinks and is absorbed, and the bones gradually coalesce; by which a further advantage is derived, that of an accession of strength to the cranium at large.

iis locis conglutinantur, verum tamen non nisi in embrionibus ad fonticulos, ut aiunt, hæc linea notabili latitudine, observatur. Ossibus enim capitis hic locorum cerebro crescente, placide quasi deductus, cartilago augetur, latior evasura, nisi pristina pars simul in os mutaretur, inde ossa calvariæ, eodem modo, quo ossa longa deductis epiphysibus, vel quod unum idemque est, marginibus crescere, liquet, etsi in ossibus, longis sutura epiphyses inter et diaphysin non crispetur.

"Quo junior igitur infans, eo minus crispa et implexa sutura, vel ut rectius loquar, linea cartilaginosa angusta, ossa jungens, observatur. Quum vero aucta ætate ossa, crescente cerebro, deducuntur, eorumque, crassitudo adposita cum internæ, tum externæ potissimum tabulæ, (internæ enim incrementum citius absolvi videtur) massa ossea, augetur, non potest non esse, quin hæc crispa suturæ forma, quum quidem nasci cœpit, externâ in superficie tamdiu, augeatur, donec tandem ipsa ea quam maxime impediat, quo minus cerebrum calvariam ulterius deducere possit, quod pubertatis tempore accidit. Rarissime hæc ossificatio ad ætatem virilem usque detinetur."—Soemmerring de corporis Humani Fabrica, page 912.

If

If any additional argument be neceſſary in ſupport of this opinion, I may alſo notice the ſtriking analogy which ſubſiſts between the ſeparation of one bone of the ſkull from another by a future; and that ſeparation which exiſts between the body of a cylindrical bone and its epiphyſis. They each remain only for a certain length of time; each allows the interpoſition of a ſecretory organ; and both begin to be obliterated when the bones with which they are connected have completed their growth, and their continuance is no longer neceſſary.

XIV.

On the Reproduction of Buds. By THOMAS ANDREW KNIGHT, *Eſq. F. R. S.**

MY DEAR SIR,

EVERY tree in the ordinary courſe of its growth generates, in each ſeaſon, thoſe buds which expand in the ſucceeding ſpring; and the buds thus generated, contain, in many inſtances, the whole of the leaves which appear in the following ſummer. But if theſe buds be deſtroyed during the winter or early part of the ſpring, other buds, in many ſpecies of trees, are generated, which in every reſpect perform the office of thoſe which previouſly exiſted, except that they never afford fruit or bloſſoms. This reproduction of buds has not eſcaped the notice of naturaliſts; but it does not appear to have been aſcertained by them from which, amongſt the various ſubſtances of the tree, the buds derive their origin.

If the generated buds of a ſeaſon be deſtroyed, others are produced.

Du Hamel conceived that reproduced buds ſprang from pre-organized germs; but the exiſtence of ſuch germs has not, in any inſtance, been proved, and it is well known that the roots, and trunk, and branches of many ſpecies of trees will, under proper management, afford buds from every part of their ſurfaces; and therefore, if this hypotheſis be well founded, many millions of ſuch germs muſt be annually generated in every large tree; not one of which in the ordinary courſe of nature will come into action: and as nature, amidſt all its exu-

Du Hamel's opinion that theſe laſt are from pre-organized germs.

Objection.

* Phil. Tranſ. 1805.

berance

berance, does not abound in ufelefs productions, the opinions of this illuftrious phyfiologift are, in this cafe, probably erroneous.

Suppofition that they are afforded by the bark.

Other naturalifts have fuppofed the buds, when reproduced, to fpring from the plexus of veffels which conftitutes the internal bark; and this opinion is, I believe, much entertained by modern botanifts: it neverthelefs appears to be unfounded, as the facts I fhall proceed to ftate will evince.

Inftance to the contrary in fea cale. Internal buds.

If the fruit-ftalks of the fea cale (*crambe maritima*) be cut off near the ground in the fpring, the medullary fubftance, within that part of the ftalk which remains attached to the root, decays; and a cup is thus formed in which water collects in the fucceeding winter. The fides of this cup confift of a woody fubftance, which in its texture and office, and mode of generation, agrees perfectly with the alburnum of trees; and I conceive it to be as perfect alburnum, as the white wood of the oak or elm: and from the interior part of this fubftance, within the cup, I have frequently obferved new buds to be generated in the enfuing fpring. It is fufficiently obvious that the buds in this cafe do not fpring from the bark; but it is not equally evident that they might not have fprung from fome remains of the medulla.

Potatoes afford buds at the cut furface,

In the autumn of 1802, I difcovered that the potatoe poffeffed a fimilar power of reproducing its buds. Some plants of this fpecies had been fet, rather late in the preceding fpring, in very dry ground, where, through want of moifture, they vegetated very feebly; and the portions of the old roots remained found and entire till the fucceeding autumn. Being then moiftened by rain, many fmall tubers were generated on the furfaces made by the knife in dividing the roots into cuttings; and the buds of thefe, in many inftances, elongated into runners, which gave exiftence to other tubers, fome of which I had the pleafure to fend to you.

—and therefore not from the bark.

I have in a former paper remarked, that the potatoe co[illegible] of four diftinct fubftances, the epidermis, the true fkin, the bark, and its internal fubftance, which, from its mode of formation, and fubfequent office, I have fuppofed to be alburnous: there is alfo in the young tubes a tranfparent line through the centre, which is probably its medulla. The buds and runners fprang from the fubftance which I conceive to be the alburnum of the root, and neither from the central part of it, nor from the

the furface in contact with the bark. It muft, however, be admitted, that the internal fubftance of the potatoe correfponds more nearly with our ideas of a medullary than of an alburnous fubftance, and therefore this, with the preceding facts, is adduced to prove only that the reproduced buds of thefe plants are not generated by the cortical fubftance of the root: and I fhall proceed to relate fome experiments on the apple, and pear, and plumb-tree, which I conceive to prove that the reproduced buds of thofe plants do not fpring from the medulla.

Other inftances of fruit trees in which reproduced buds appeared to fpring from the alburnum.

Having raifed from feeds a very confiderable number of plants of each of thefe fpecies in 1802, I partly difengaged them from the foil in the autumn, by digging round each plant, which was then raifed about two inches above its former level. A part of the mould was then removed, and the plants were cut off about an inch below the points where the feed-leaves formerly grew; and a portion of the root, about an inch long, without any bud upon it, remained expofed to the air and light. In the beginning of April, I obferved many fmall elevated points on the bark of thele roots, and, removing the whole of the cortical fubftance, I found that the elevations were occafioned by fmall protuberances on the furface of the alburnum. As the fpring advanced, many minute red points appeared to perforate the bark: thefe foon affumed the character of buds, and produced fhoots, in every refpect fimilar to thofe which would have fprung from the organized buds of the preceding year. Whether the buds thus reproduced derived any portion of their component parts from the bark or not, I fhall not venture to decide; but I am much difpofed to believe that, like thofe of the potatoe, they fprang from the alburnous fubftance folely.

They do not originate from the medulla.

The fpace, however, in the annual root, between the medulla and the bark is very fmall; and therefore it may be contended that the buds in thefe inftances may have originated from the medulla. I therefore thought it neceffary to repeat fimilar experiments on the roots and trunks of old trees, and by thefe the buds were reproduced precifely in the fame manner as the annual roots: and therefore, conceiving myfelf to have proved in a former Memoir,* that the fubftance which has

* Phil. Tranf. of 1803.

beer

been called the medullary procefs does not originate from the medulla, I muft conclude that reproduced buds do not fpring from that fubftance.

Remarks on the manner in which this procefs of nature is probably effected.

I have remarked, in a paper which you did me the honour to lay before the Royal Society in the commencement of the prefent year, that the alburnous tubes at their termination upwards invariably join the central veffels, and that thefe veffels, which appear to derive their origin from the alburnous tubes, convey nutriment, and probably give exiftence to new buds and leaves. It is alfo evident, from the facility with which the rifing fap is transferred from one fide of a wounded tree to the other, that the alburnous tubes poffefs lateral as well as terminal orifices: and it does not appear improbable that the lateral as well as the terminal orifices of the alburnous tubes may poffefs the power to generate central veffels; which veffels evidently feed, if they do not give exiftence to, the reproduced buds and leaves. And therefore, as the preceding experiments appear to prove that the buds neither fpring from the medulla nor the bark, I am much inclined to believe that they are generated by central veffels which fpring from the lateral orifices of the alburnous tubes. The practicability of propagating fome plants from their leaves may feem to ftand in oppofition to this hypothefis; but the central veffel is always a component part of the leaf, and from it the bud and young plant probably originate.

Attempt to difcover the fame power in feeds.

I expected to difcover in feeds a fimilar power to regenerate their buds; for the cotyledons of thefe, though diffimilar in organization, execute the office of the alburnum, and contain a fimilar refervoir of nutriment, and at once fupply the place of the alburnum and the leaf. But no experiments, which I have yet been able to make, have been decifive, owing to the difficulty of afcertaining the number of buds previoufly exifting within the feed. Few, if any, feeds, I have reafon to believe contain lefs than three buds, one only of which, except in cafes of accident, germinates; and fome feeds appear to contain a much greater number. The feed of the peach appears to be provided with ten or twelve leaves, each of which probably covers the rudiment of a bud, and the feeds, like the buds of the horfe-cheftnut, contain all the leaves and apparently all the buds of the fucceeding year: and I have never been able to fatisfy myfelf that all the buds were eradicated without having

having destroyed the base of the plumule, in which the power of reproducing buds probably resides, if such power exists.

Annual and biennial plants have not this power.

Nature appears to have denied to annual and biennial plants (at least to those which have been the subjects of my experiments) the power which it has given to perennial plants to reproduce their buds; but nevertheless some biennials possess, under peculiar circumstances, a very singular resource, when all their buds have been destroyed. A turnip, bred between the English and Swedish variety, from which I had cut off the greater part of its fruit-stalks, and of which all the buds had been destroyed, remained some weeks in an apparently dormant state; after which the first seed in each pod germinated, and bursting the seed-vessel, seemed to execute the office of a bud and leaves to the parent plant, during the short remaining term of its existence, when its preternatual foliage perished with it. Whether this property be possessed by other biennial plants in common with the turnip, or not, I am not at present in possession of facts to decide, not having made precisely the same experiment on any other plant.

Correction of a former inference.

I will take this opportunity to correct an inference that I have drawn in a former paper,* which the facts (though quite correctly stated) do not, on subsequent repetition of the experiment, appear to justify. I have stated, that when a perpendicular shoot of the vine was inverted to a depending position, and a portion of its bark between two circular incisions round the stem removed, much more new wood was generated on the lower lip of the wound become uppermost by the inverted position of the branch, than on the opposite lip, which would not have happened had the branch continued to grow erect; and I have inferred that this effect was produced by sap which had descended by gravitation from the leaves above. But the branch was, as I have there stated, employed as a layer, and the matter which would have accumulated on the opposite lip of the wound had been employed in the formation of roots, a circumstance which at that time escaped my attention. The effects of gravitation on the motion of the descending sap, and consequent growth of plants, are, I am well satisfied, from a great variety of experiments, very great; but it will be very difficult to discover any method by which the extent

* Phil. Trans. of 1803.

of its operation can be accurately aſcertained. For the veſſels which convey and impel* the true ſap, or fluid from which the new wood appears to be generated, paſs immediately from the leaf-ſtalk towards the root; and though the motion of this fluid may be impeded by gravitation, and it be even again returned into the leaf, no portion of it, unleſs it had been extravaſated, could have deſcended to the part from which the bark was taken off in the experiment I have deſcribed. I am not ſenſible that in the different papers which I have had the honour to addreſs to you, I have drawn any other inference which the facts, on repetition of the experiments, do not appear capable of ſupporting.

I am, &c.

THO^s. ANDREW KNIGHT.

Elton, May 12, 1805.

XV.

Experiments on the Gaſeous Oxide of Azote, by a Society of Amateurs at Toulouſe. Publiſhed by M. P. DISPAN, *Profeſſor of Chemiſtry in the College of that City.*†

Diſagreement of former experiments on the oxide of azote.

THE motive for the following experiments was the very different, and even contradictory reſults, which have been publiſhed of former effects. The experiments were tried upon more than a dozen perſons, and in ſome caſes repeated two or three times; the ſenſations which each experienced were written down at the moment, by the reporter, from whoſe memorandums the ſubſequent obſervations are drawn.

Preparation of the nitrate of ammonia.

The nitrate of ammonia uſed for the experiment was indiſtinctly cryſtallized, but was quite neutral. Its taſte was very pungent, with a ſlight odour. It had been formed by the ſaturating very pure nitric acid with ammoniacal gas obtained by diſtilling ſal ammoniac with the common potaſh of commerce.

Proceſs for obtaining the gaſeous oxide of

About one hectogramme (1545 grains) of this ſalt was put into a ſmall retort, and placed on a ſand-bath, where the ſalt

* Phil. Tranſ. of 1804.

† Annales de Chimie, Vol. LVI. p. 243.

melted

melted and boiled for a ſhort time without yielding any gas; at length, the retort became filled with a white vapour, which quickly diſappeared; the gas was then rapidly diſengaged, and was caught in bladders. By degrees the diſengagement became more and more ſlow, and when the operation was ended, ſcarcely any thing remained in the retort.

Another experiment was made with a larger retort, and three hectogrammes (10 oz. troy) of the ſalt, from which was obtained gas ſufficient to fill eight bladders. This operation proceeded in a ſimilar manner with the former; except that as the retort cooled, a red vapour aroſe within it, which it was aſcertained by experiment, contained no nitrous gas. The ſame proceſs on a larger ſcale.

Effects of Gaſeous Oxide of Azote when breathed into the Lungs.

All who have taſted or inhaled this gas, agree in deſcribing its flavour as ſtrongly ſaccharine, and remaining upon the organs of ſome perſons during the whole day after receiving it. M. Diſpan obſerved in it an after-taſte of nitre; but acknowledges that it was the laſt collected gas which he taſted.—M. de M***, perhaps under a ſimilar impreſſion, ſays he perceived in it a ſtyptic quality. The gas has a ſaccharine taſte.

The method of reſpiring this gas was by means of a bladder with a ſtop-cock in it, applied to the mouth; the noſtrils being cloſed, and the lungs as much as poſſible emptied. The gas was inſpired.

No. 1. The firſt perſon upon whom the experiment was tried, ſwooned at the third inſpiration, and remained ſenſeleſs about five minutes, when he recovered, but with a ſenſation of great fatigue. He recollected to have experienced only a ſudden faintneſs, attended with a tingling at the temples.

No. 2. M. de M*** obſerved a ſaccharine and ſtyptic taſte, and experienced a ſenſe of great dilatation, accompanied with heat in the breaſt; his veins ſwelled, and his pulſe was quickened: ſurrounding objects ſeemed to revolve round him. But he thought he could have borne a ſtronger doſe; the bladder not being large enough for his lungs.

No. 3 experienced a ſaccharine taſte on the firſt inſpiration; but became inſenſible to thoſe which ſucceeded. His lungs were forcibly dilated with great heat. When the bladder was removed, he appeared very comfortable, but could not refrain from violent burſts of involuntary laughter.

No.

No. 4 had the same saccharine taste with the preceding, and retained the impression from ten o'clock in the morning till after midnight. He experienced vertigoes, and his legs trembled under him during the remainder of the day.

No. 5, the same saccharine taste. On quitting the bladder, he had a dizziness of sight, which was succeeded by a sensation of great pleasure throughout the body. His legs were weakened.

No. 6. Saccharine flavour throughout the day; tingling in the ears; legs tottering, and the stomach oppressed. All that he experienced was rather painful than agreeable.

Receiving the gas from a bladder, had no influence on the result of the experiment.

In order to ascertain what influence the mode of breathing from a bladder might have on the foregoing results, the parties were requested to inspire common air in the same manner.—They were all mechanically fatigued by it, and nothing more.

Oxigen gas differed from common air only by a small increase of the heat of the lungs.

The bladders were next filled with oxygen gas, and applied as before to the same persons, who found only a slight difference between it and common air, consisting in an augmentation of the heat of the lungs.

Conclusion.

The singular effects above described, can, therefore, and ought only to be ascribed to the gaseous oxide of azote.

Other experiments.

Another meeting of the society was held for repeating the experiments more at large, on the respiration of gaseous oxide of azote.

Description of the apparatus with upwards of 2lbs. of the salt.

Eight hectogrammes (27½ oz. troy) of nitrate of ammonia, prepared as before, were put into a retort, with its neck fitted to a double-bodied receiver, from whence, by means of a tube of welter, the gas passed into an inverted vessel over water. The retort was placed on a sand-bath.

Particulars of the process.

As soon as the heat affected the retort, the salt melted; and nearly at the same moment, sparkling vapours arose in the retort, but in very small quantity. The air which the heat expelled from the vessels had a nitrous odour; but this as well as the vapours gradually diminished, and as the process continued they disappeared altogether; they were succeeded by a lively smell of prussic acid. At length the retort became filled with white vapours, and the gaseous oxide of azote began to pass over. The disengagement soon became so abundant that it was judged proper to draw out the fire; but afterwards, on replacing the coals, the gas, which in the interval had diminished,

nished, was again so rapidly developed that the luting of the vessels began to give way. But notwithstanding the loss which this occasioned, the disengagement continued extremely rapid in the receiver for at least a quarter of an hour.

Danger of explosion.

M. Dispan supposes, that if the luting had not given way, an explosion would have taken place, as has happened to others in this process.

He next proceeds to state the effects of the respiration of this gas.

The effects produced by the last gas were more powerful than the former.

Twelve persons underwent the experiment, and on many was repeated. He observes that most of them had inhaled the gas of the former operation, where two out of seven experienced pleasing sensations; but on this second occasion, not one felt pleasure; on the contrary, they all felt pain, and many suffered extremely.

One person stamped with his foot the whole time of the breathing: when the bladder was removed, he recovered from the profound stupor into which he had been plunged, and complained of a pain in the back part of his head, as if he had received a violent blow from a dagger: he could not be prevailed on to make another trial. The other persons in general were affected with vertigoes and dizziness of sight, succeeded in some by involuntary convulsive fits of laughter.

M. Dispan's description of the effect of the gas upon himself.

M. Dispan tried the effects of this gas on himself, which he thus describes:—

"At the first inspiration, I emptied the bladder, and my mouth was instantaneously filled with a saccharine flavour, which extended into my lungs and inflated them. I emptied and filled them again; but on the third attempt, my ears were filled with a tingling noise, and I dropped the bladder. I did not, however, become altogether insensible, but remained in a kind of benumbed astonishment, rolling my eyes about without fixing them on any particular object: I was then suddenly seized with convulsive laughing fits, such as I never in my life before experienced. In a few seconds this propensity to laugh stopped suddenly, and I no longer felt any unpleasant symptom."

Effects on two other persons.

Two others on whom the gas was tried, experienced only a convulsive movement of some of the muscles of the face; but were in the course of the day attacked with violent diarrhœa.

Difficulty of reducing the effects of this gas to any general system.

M. Dispan thinks it will be very difficult to reduce the effects of gaseous oxide of azote to any general system, as they vary so considerably in their operations upon different individuals, and, what is more singular, even upon the same person.

M. D. concludes his paper with an account of an experiment to ascertain the effect of gaseous oxide of azote upon animals.

Experiments on a bird immersed in gaseous oxide of azote.

He placed a greenfinch in a vessel of sufficient dimensions, and filled it with gaseous oxide of azote. At first, the bird seemed to suffer no inconvenience; but he soon gradually closed his eyes, and dropped gently on his side, as if asleep. On being restored to the pure air, he resumed his feet, without attempting to fly away. About an hour afterwards he was subjected to a second trial, and having been suffered to remain longer in the vessel, he was taken out quite dead.

M. Dispan thinks it very remarkable that the bird should make no effort to escape, and that he should manifest no convulsive symptoms, such as take place in experiments with other gases.

XVI.

Observations on the Mammoth, or American Elephant, by which it is proved to have been an herbivorous Animal. In a Letter from the Right Reverend Bishop MADISON.*

Discovery of a mammoth having vegetable remains in its stomach.

ONE of those facts has lately occurred, which the naturalist knows best how to appreciate, and which I therefore take a pleasure in communicating to you. It is now no longer a question, whether the Mammoth was a herbivorous or carnivorous animal. Human industry has revealed a secret, which the bosom of the earth had, in vain, attempted to conceal.—In digging a well, near a Salt-Lick, in Wythe-county, Virginia, after penetrating about five feet and a half from the surface, the labourers struck upon the stomach of a mammoth. The contents were in a state of perfect preservation, consisting

* To Benjamin Smith Barton, M. D. editor of the Philadelphia Medical and Physical Journal, from which (vol. II.) it is taken.

of

of half masticated reeds, twigs, and grafs, or leaves. There could be no deception; the fubftances were defignated by obvious characters which could not be miftaken, and of which every one could judge; befides, the bones of the animal lay around, and added a filent, but fure confirmation. The whole refted upon a lime-ftone rock. I have not feen, as yet, any part of thofe contents; for, though I was within two days' journey of the place where they were found, I was fo well fatisfied with the narration of gentlemen who had feen them, and upon whofe veracity, as well as accuracy, I could rely, that I thought the journey unneceffary; efpecially as I took meafures to enfure the tranfmiffion of a fufficient quantity of the contents, together with all the bones, to Williamfburgh. When the contents arrive, a part fhall be forwarded to you. I hope to form a complete fkeleton of this vaft animal, having given directions to fpare no labour, in digging up every bone.

We fhould not be furprifed, that thefe fubftances fhould be thus preferved, when we recollect the ftate of the rhinoceros, mentioned by Pallas. Blumenbach, in his *Manuel d'Hiftoire Naturelle*, vol. II. p. 398, (traduit par Artaud), has a note, which is very applicable to the prefent fubject. He fays, " Quelquefois on trouve encore des pièces animales qui ont confervé, fans alteration, leurs parties *molles;* mais, cependant, comme elles se trouvent auffi enfouies dans la terre par la fuite de ces grandes cataftrophes des temps antérieurs, on doit les ranger parmi les corps pétrifiés, dans le fens le plus étendu. Je citerai, par exemple, le rhinocéros deterré près de Wiloi, en Sibérie, qui offroit encore des reftes tres-reconnoiffables, même ayant encore l'odeur animal de mufcles, de chair, de peau, & de poils. Pallas l'a décrit tres-exactement dans les Nov. Comment. Petropolit., tome 13. p. 585."

Remarks on the preservation of thofe bodies,

Whether this firft kind of petrifaction, of which Blumenbach fpeaks, and which he calls *fimplement calcinés*, has been the caufe of the prefervation of thefe fubftances, or whether it be the effect of the marine falt, with which the earth, where they were buried, has been conftantly charged, muft be left to future inveftigation. I pretend not to decide. Had they been buried deep in the earth, that circumftance alone might have prevented a decompofition; but the depth of five or fix feet feems infufficient to arreft that chemical action, which

—probably from marine falt.

 changes

changes the appearances of organized bodies. The fact, however, is decisive, as to the principal question. It has summoned the discordant opinions of philosophers before a tribunal, from which there is no appeal.

Williamsburgh, October 6th, 1805.

Note on the preceding Paper. By the Editor.

Facts by Mr. Nevil on long preserved vegetable bodies.

Mr. Francis Nevil, in his account of the elephantine teeth that were discovered in the north of Ireland, early in the eighteenth century, has mentioned some facts relative to the long preservation of vegetable matters, which seem worthy of our notice in this place: and the more so, as this gentleman's paper seems not to have excited any attention among the modern writers on the exuviæ of animals found in countries in which the living animals themselves are no longer seen. Some extravagant conjectures are mixed with Mr. Nevil's account: but these do not, in the least, invalidate the truth of what he says, relative to the bed upon which the Irish elephant was laid.

His narrative.

"The place (says he) where this monster lay, was thus prepared, which makes me believe it had been buried, or that it had lain there since the deluge. It was about four feet under ground, with a little rising above the superficies of the earth, which was a plain under the foot of a hill, and about thirty yards from the brook * or thereabout. The bed whereon it lay had been laid with fern, with that sort of rushes here called sprits, and with bushes intermixed. Under this was a stiff blue clay on which the teeth and bones were found: above this was first a mixture of yellow clay and sand much of the same colour; under that a fine white sandy clay, which was next to the bed: the bed was for the most part a foot thick, and in some places thicker, with a moisture clear through it; it lay sad and close, and cut much like turf, and would divide into flakes, thicker or thinner as you would; and in every layer the seed of the rushes was as fresh as if new pulled, so that it was in the height of seed-time that those bones were laid there. The branches of the fern, in every

* "A small brook that parts the counties of Cavan and Monaghan."

ly as we opened them, were very distinguishable, as were the seeds of the rushes and the tops of the boughs. The whole matter smelt very sour as it was dug, and tracing it I found it 34 feet long and about 20 or 22 feet broad."—"I forgot to mention that there was a great many nut-shells found about the bed, perhaps those might have been on the bushes which composed part of the bed *."

XVII.

Observations on the Danger of using Earthen-ware or Pottery of a bad Quality. By M. Boidevin of Rouen †.

PURE white argil forms the body of the finest pottery which bears the name of porcelain; clays less pure, and coloured more or less with iron, serve to form the stone ware, or hard earthen-ware, and the common or soft sort, which differs from the other, in not experiencing a commencement of fusion at their surface in baking, like porcelain or stone ware. Different kinds of pottery.

This badly prepared common earthen-ware is the kind which is occasionally attended with danger in its use, and is the subject of this paper.

Earthen-ware.

The biscuit of brown earthen-ware is prepared from a ferruginous clay; that of white earthen-ware is composed of a mixture of ferruginous clay, of another clay containing much silicious sand, a little lime, and finally of a porous clay, which renders it less compact, and gives it whiteness after baking. Common brown ware.

Nature not always affording these earths in the same state of combination, occasions differences in the biscuit, when it becomes subjected to the heat: other differences also arise in the action of the enamel on the biscuit. If the earth be too ferruginous, or too much mixed with silicious particles, the enamel, during the baking, acts as a flux on the biscuit, softens it, and occasions the pieces to lose their shape. Differences in the ware from the quality the materia

If the earth is too porous it absorbs the enamel and remains rough,

* A Natural History of Ireland, in three parts, by Dr. Gerrard Boate, Thomas Molineux, M.D. F.R.S. and others. Pages 128—130. Dublin: 1755.

† Annales de Chemie, T. 55.

rough, and as it were dried. If it contains too much lime, it throws off the enamel, which falls from it in scales instead of adhering to it.

Compofition of the enamel or glaze.

On the other hand, the white enamel is compofed of filicious fand, a little lime, lead and tin oxides, and fome flux, ground together with water in mills. The brown fort is compofed of the fame materials, with the addition of manganefe and perigord ftone *.

Caufes which occafion variations in the glaze.

The greater or lefs fufibility of the fand; the greater or lefs purity of the lead, of the tin, and of the faline fubftances employed as fluxes; the different degrees of heat which the mixture receives in the glazing; the variations of the finenefs given to the glazing materials by the action of the mill, are fo many circumftances which caufe changes in the enamel in its ftate of fufion on the pieces, relative to the ftate in which it finds the bifcuit and to the fufible layer, with which this laft is covered.

Pottery.

Brown pottery.

The body of the brown pottery is a red clay, more or lefs ferruginous and compact according to the places where it is procured.

Yellow pottery.

The common or yellow pottery is made of a white clay, which contains a little lime and magnefia, and a confiderable quantity of filicious fand, which may be generally efteemed a fourth of the mafs.

Glaze for brown ware.

The glazing of the brown pottery is formed with a mixture of filicious fand, yellow or red oxide of lead, and manganefe pulverifed together.

Glaze for yellow ware.

That of the yellow earthen-ware is compofed of a mixture of filicious fand, and red oxide of lead, which, during its baking vitrifies at its furface, and forms a yellow glazing more or lefs tranfparent. To this mixture is commonly added, in France, a little oxide of magnanefe in powder, more or lefs fine, without grinding them together. This is called the *grain*, becaufe it fufes more difficultly than the other materials, without mixing with them, and by that means forms ftreaks, fpots, or brown fpecks, according to the coarfenefs of the powder itfelf.

Mottled ftreaks in foreign ware.

Cloudy tinges in the glaze.

In fome manufactories they mix oxide of copper with the common glazing, to give it a green colour, and in others they form defigns on the pieces, with oxide of copper, which pro-

* A black ftone or compact manganefe. T.

duces

duces a green, with oxide of iron, which causes a red, or with oxide of manganese, which gives a brown.

Great imperfections are produced in pottery, from the injudicious use of glazing over earths of an unsuitable nature, and this is more remarkable when the earths are not so well prepared for their glazings as they are for those of the finer wares. The articles of common pottery are less carefully prepared both in their materials and baking. This last is usually performed at a single operation, and with less fire. Causes of imperfection in pottery.

The means of producing good pottery and earthen-ware, consist in carefully chusing the earths for forming the body: in producing an exact coincidence of expansion by heat between them, and the vitrifiable glaze with which they are to be covered, and in baking them by a proper degree of fire, produced from combustibles not capable of changing the nature of the glazing. Cautions requisite to insure its goodness.

The neglect of these attentions occasion defects in the manufactured articles, which are either unsightly and nothing more, or both unsightly and dangerous.

The unsightly defects which are found in ill conditioned pottery or earthen-ware, are, *scaling*; the *dropping* or *drops*; *smoke*; *drying* of the ware, and *flaws* or *cracks*. Defects or deformities enumerated.

The scaling is the appellation used when the glazing of a piece detaches itself in scales, by the action of moist air, or on the least touch, and leaves the biscuit uncovered. Scaling of the glaze.

The *dropping* or *drops* take place when the moisture of the fuel having struck the pieces during the baking, the enamel is collected in *drops* on the surface, and remains vitrified in that form, instead of being equally spread. Dropping or drops.

The smoky appearance happens when a piece has not been purified by a clear flame, but remains blackened or stained. Smoky tinge.

The drying happens when the pieces are, as it were, roasted in the firing, and come out rough from the absorption of the enamel into their substance. Drying.

The flaws happen, when the earth or the biscuit, having a different pyrometrical expansibility from that of the enamel; the body contracts in cooling more than the glaze which is therefore split, or which is divided into an infinite number of small parts, sometimes not perceptible to the eye when the pieces are new, but which become very visible, when the goods have imbibed any greasy substance in using. Flaws.

All these defects, though disagreeable to the eye, have really, The coarse pottery is most defective.

really, with regard to the ware itself, only the inconvenience of a dirty appearance, provided the biscuit is always compact, and well baked. But it is different in the common pottery in which the dropping, the scaling, and the flaws produce more injurious defects. As the earth is more porous and less baked in those, the liquids preserved in them enter into the pores where they become altered and decomposed, and produce sulphurated hydrogen, which injures every thing kept in them.

Cavities or pits from bad firing.

The most noxious defects in pottery are the cavities or pits, and the underbaking. The pits are roughnesses or hollow bubbles which are found on those pieces, whose enamel being injured by rubbing, or being too little acted on by the fire, has not been fused into a vitreous substance. In these the metallic oxides are in a state capable of doing injury, being still soluble in fat or acid substances.

Underbaking or imperfect fusion of the

The underbaking occasions one of the most dangerous defects in pottery; the pieces thus affected have not had sufficient heat to cause the enamel to do more than agglutinate together, and in some cases it even still remains in powder. It is therefore capable of being divided, and taken up by all the liquids with which it may come in contact.

It is easy to shew the danger to which the public must be exposed in buying those articles at a low price which are called waste or refuse and which ought to be carefully thrown away. In vain may it be said that they are used daily without any immediate mischief happening; from the injury being more concealed, it is no less destructive. It is known that lead and its oxides act insensibly on the organs of digestion, especially when taken in small quantities: They do not, however, less certainly cause, at length, emaciation, cholics, convulsions, sometimes of all parts of the body, with obstinate diarrhœas; and the wretched people who use such vessels become the victims of their own ignorance, and of the imprudent avarice of the manufacturer.

It would be to the honour of enlightened manufacturers, not to offer to the public pieces which have imperfections beyond a certain degree, and to make this sacrifice to the good of national commerce, especially as they can avoid the loss by a greater attention to their materials.

XVIII. Extract

XVIII.

Extract of a Letter from M. JOHN MICHAEL HAUSSMANN, to M. BERTHOLLET, on the Existence of intermediate Terms of Oxidation. *

I THINK there are sufficient grounds for admitting, with you, that there exist, in the oxidation of many metallic bodies, intermediate degrees between the *minimum* and the *maximum*. Existence of intermediate degrees of oxidation of metals.

The first example I shall cite, is, that of a *minimum* oxide of tin, precipitated from the muriatic solution, and dissolved in an excess of caustic potash; a metallic alkaline solution which I have before noticed in my Observations on the Red Dye of Adrianople, inserted in the "*Annales de Chimie*," and also in a Memoir on the coloured Oxides of Tin, inserted in the "*Journal de Physique*." Oxide of tin.

By avoiding any dilution of the muriate of tin, and using a very concentrated solution of caustic potash, the mixture disengages much caloric, part of the tin is precipitated in the metalline state, whilst the remainder is held in solution in an intermediate state of oxidation. This alkaline solution is so disoxidant, that it changes the yellow oxide of gold, fixed on cotton, by means of ammonia, to a grey; whilst a similar yellow pattern underwent no change of colour on being steeped in the simple liquor of caustic potash. A like alteration took place on dipping a cotton cloth, which had been previously stained with the solution of gold, and well dried in the alkaline solution of tin, which also produced the same effect on pouring into it the pure solution of gold diluted with water. Experiment. Muriate of tin is in part precipitated metallic and the rest suspended with intermediate oxidation.

This change of the yellow colour of oxide of gold by the alkaline solution of tin, is not the only proof of an intermediate state of oxidation; this liquor possesses besides, a property of destroying the blackish-brown colour of the oxide of manganese stained upon cotton by an alkaline precipitant. Other proofs of that intermediate state.

All these changes are more rapidly produced, if, prior to the precipitation and solution in the caustic potash liquor, the muriatic solution of tin be diluted with six or eight parts of water, in which case there is no sensible disengagement of

* Annales de Chimie, Vol. LVI. 5.

caloric

caloric, and no tin is precipitated in the metalline state. This solution, whose oxidation approaches the degree of *minimum*, for the most part retains an aqueous transparency, without any precipitation of oxide; even when long exposed to the atmospheric air, it does not lose the property of changing the yellow oxide of gold to a grey colour, or of destroying the blackish brown tint of oxide of manganese, when fixed upon cotton.

Oxide of manganese.

The oxide of manganese is capable of various degrees of oxidation; if a piece of cotton cloth be dipped in the transparent solution of sulphate of manganese, it will, when dry, retain its original whiteness; but on their dipping the same cloth in the liquor of carbonated or caustic potash, it will, after washing and exposure to the atmospheric air, be coloured brown; which colour will acquire a deeper shade, approximating to black, on being steeped for a time in an oxigenated alkaline, muriatic liquor. The oxigenated alkaline liquor, on being for any length of time submitted to the action of the brown precipitate of manganese, instead of the rag steeped therein (which is to dissolve by means of an increased oxidation) will assume a purple colour, of greater or less transparency as the time of their union has been longer or shorter.

Other oxides.

There seems reason, generally, to expect particular results from submitting any of the metallic oxides to the action of this oxigenated muriatic alkaline liquor; which might, perhaps, be a means of giving them acid properties, and at the same time of proving the gradual oxidation of many metals; this is the more observable in white oxide of lead, which becomes gradually coloured by long exposure to the oxigenated liquor, and being frequently stirred.

Muriatic and nitro-muriatic solutions of tin, though colourless in themselves, acquire by admixture a vinous tint.

Muriatic and nitro-muriatic solutions of tin, well diluted with water, have an aqueous transparency, when properly made; but if the two be mixed together, they acquire a fine vinous colour, similar to that of Malaga; this can only arise from the oxigen of the nitro-muriatic being in part communicated to the muriatic solution of tin.

The addition of solution of gold produces a purple dye;

If a solution of gold with great excess of acid, and diluted with from 130 to 160 parts of water, be gradually poured into the above mixture, stirring it all the time, the intensity of the colour will be increased, till at length the liquor becomes

comes of a beautiful purple hue, in which all kinds of goods may be dyed; this may be changed to the tint of peach or lilac blossoms, by increasing the proportions of the nitro muriatic solution; or, on the other hand, by causing the muriatic solutions of tin to preponderate; shades of grey will be obtained, deeper or ler in colour, according to the quantity of the solution ad Care must, however, be taken, in the latter experiment, that too great a proportion of the muriatic liquor of tin be not used; for by depriving the oxide of gold of too much of its oxigen, it might be too much disoxided and precipitated. The precipitate caused by such an accident is not altogether void of oxigen, which prevents its gilding cold silver, as do the ashes of burned cloth impregnated with the solution of gold. The degree to which the preservation of the tincture of gold may be carried, must depend on the proportions of the two solutions of tin, their being more or less surcharged with acids, and the quality of the solution of gold, wherein also there should be a very great excess of acid.

capable of being changed to the bloom of peaches or lilac; or even to grey, by proportioning the quantity of the two solutions of tin.

The precipitate of the oxide of gold will not gild silver, without the assistance of heat.

The purple tincture of gold, though of the most perfect transparency, is decomposed by exposure to a strong heat, and throws down what is known by the name of "*Purple of Cassius*," whose beauty depends on the quantity of nitro-muriatic solution of tin made use of. The latter, however, if mixed alone with the solution of gold, without the presence of muriate of tin, produces no alteration of colour, and, if the mixture be not too much weakened with water, is a very long time before it gives a precipitate.

Purple powder of Cassius.

The purple tincture of gold, is, properly speaking, nothing more than the powder of cassius, held in solution by means of the oxigen of the nitro-muriatic liquor of tin; and there is every reason to believe, that in the powder of cassius, the oxide of gold is in some way combined with the oxide of tin, which, by transmitting to it its own origin, during its fixation upon porcelain, prevents it, I think, from returning to its metallic state. I find a difficulty in subscribing to the opinion of Dr. Richter, of Berlin, who, in a memoir (which I have not read) attempts to prove, by mathematical demonstration, that the crimson-coloured gold on porcelain is in the metallic state.

Dr. Richter's opinion of the crimson-coloured gold upon porcelain.

The purple tincture of gold might be advantageously employed in dying silks, without greatly enhancing the price. The

Purple tincture of gold surpasses all others in dying silks.

The colour obtained from it surpasses all others in duration, since nothing less than combustion can destroy it. It is necessary, however, to leave the silk a long time in this dye; and the depth of the shade will be in proportion to the number of times the article is dipped; it must be well wrung, rinced, and dried, between each immersion.

The gradation of shades already noticed are indications of a gradual oxidation.

The gradation of colours produced by mixture of the nitro-muriatic, and muriatic solutions of tin, being much weakened by dropping solution of gold in a great excess of acid, considerably diluted with water, into the mixture, seems to me to indicate a gradual oxidation. The acetic solution of iron proves the same truth; for on being exposed to the atmospheric air, or to the contact of oxigen gas, it gradually changes from a sea green to a reddish yellow colour. I have shewn, in a memoir on the alkaline Tincture of Mars of Stahl, that sulphate of iron may be super-oxigenated, and also lose its excess of oxigen by the action of light. On mixing concentrated sulphuric acid with nitric solution of iron, I obtained, after the nitric acid was evaporated, by leaving the residuum to imbibe the moisture of the air for several months; crystals of super-oxigenated sulphate of iron, which were at first distinguishable by their whiteness from sulphate of alumine; but the action of the light gradually tinged their surface with a yellow colour; their original whiteness, however, might, by a gentle washing, be restored at pleasure. Super-oxigenated sulphate of iron, of nearly an equal degree of whiteness, may in like manner, be obtained by precipitating nitrate of iron, and dissolving the precipitate, edulcorated and freed from water, gradually in sulphuric acid, which, if well concentrated, will produce crystals of super-oxigenated sulphate of iron without evaporation. This salt possesses an incomparable degree of astringency.

Sulphate of iron loses its excess of oxigen by ex-

The fact that linens printed with acetate of iron are liable to become rotten, a proof of the gradual transmission of oxigen.

The progress of the transmission of oxigen is more manifest on linen simply printed with acetate of iron and madder, which must be a long time exposed in the air to bleach, unless the artificial means of bleaching be adopted. The printed part of the linen frequently perishes, bearing the appearance of having been cut with a sharp instrument, or burned with concentrated acid; this, it should seem, must proceed from the action of the oxigen contained in the coloured oxide of iron, continually replenished from the atmospheric air.

It

It is not among minerals alone that substances are found which are gradually oxided, and by intermediate degrees.

Indigo affords an instance that vegetable and animal bodies offer similar proofs; for any solution of indigo (excepting the sulphate of indigo) will, on disoxidation, or on having its oxigen restored, pass through all the degrees of shade, from blueish green to very yellow olive, preserving in the mean time the same quantity of indigo in solution. The beauty and stability of the colours, either for dying or painting, will chiefly depend on the degree of oxidation. On some other occasion, Sir, I shall write to you more amply on this subject.

Vegetable and animal bodies furnish similar cases with minerals, of gradual oxidation.

SCIENTIFIC NEWS.

Mémoires de l'Académie impériale des Sciences, &c. Memoirs of the imperial Academy of Sciences, Literature, and fine Arts, of Turin, for the Years 12 *and* 13, 2 *Vols. Quarto.* 1805. *Turin.*

WHEN the Royal Academy of Turin assumed the name of Imperial, in consequence of Piedmont being annexed to France, the number of academicians was increased, to form a new class, that of literature and the fine arts. Of the two volumes published, one is appropriated to the labours of this class, the other to that of the physical and mathematical sciences.

Memoirs of the Imperial Academy of Sciences, &c. of Turin.

The latter is compiled by the secretary, Mr. Vassali Eandi, who first mentions the changes that have taken place in the list of academicians, next the various papers that have been read at their meetings, and then the books and other articles presented to the society. These lists are followed by a well written account of the labours of the academy up to the year 1805, which occupies 250 pages. After this follow the different memoirs.

1. Description and use of a new portable barometer, for measuring heights and depths, with observations made with this instrument in the circles of Turin and Saluzzo. This instrument, of which a figure is given, was invented by the secretary; who has subjoined to his paper some very curious historical notes on the places where his observations were made.

Memoirs of the Imperial Academy of Sciences, &c. of Turin.

2. Account of a waterspout that occurred in the territory of Revel, in the circle of Saluzzo, March 27, 1798, with remarks on the caufe of the phenomenon, by the fame.

3. On the different capacities for conducting heat afcertained by experiment in different articles ufed for clothing, by J. Sennebier.

4. Of a new fpecies of hawkweed, *crepis*, to which are added fome cryptogamiæ of Piedmont, by J. Baptift Balbis. A figure of this plant, which Mr. B. calls *crepis ambigua*, is given. Among the cryptogamiæ are the following new fpecies, *mucor flofculentus*, *peziza amentacea*, *lichen nivalis*. Thefe likewife are figured.

5. Experiments on the effects of the nitric and oxigenated muriatic acid, employed topically in the treatment of various difeafes, by Mr. Roffi. Mr. R. gives an account of the cure of feveral gangrenous ulcers, venereal buboes, and even contagious carbuncles, cured by the application of thefe acids.

6. Meteorological obfervations made during the folar eclipfe on the 30th of Jan. 1805, at the obfervatory of Turin, with reflections on them, by Ant. Mar. Vaffali Eandi.

7. On a fpecies of caffia, that may be fubftituted for the fenna of the fhops, by Mr. Bellardi. This is the *caffia marilandica*, which Mr. B. would call *fuccedanea*, becaufe, according to him, it may fupply the place of the *caffia lanceolata*.

8. Inquiries into the nature of the galvanic fluid, by A. M. Vaffali Eandi.

9. On the mines of plumbago in the departments of the Sture and the Po, by Mr. Bonvoifin.

10. Attempts to improve nut oil, by the fame. Mr. B. points out a method of purifying this oil, and rendering it as fit for lamps as other oils.

11. Examination of the action of the galvanic fluid on different gafes, by J. A. Giobert.

12. An anatomical and phyfiological effay on the lymphatic glands, by profeffor Roffi.

13. Solution of a problem depending on the theory of permutations and combinations, by profeffor Balbo.

14. Explanation of the circumftance of a fifh being occafionally found with prickles in the river of the 27th military divifion;

division, by Mr Giorna. The fish is the *cyprinus idus*; the male only has prickles, and loses them after spawning time. Memoirs of the Imperial Academy of Sciences, &c. of Turin.

15. A chemico-medical essay on the pulmonary consumption, by Jos. Hyac. Rizzetti. The principal subject of this essay is the nature of the matter expectorated.

The following papers are by foreign members.

1. Memoir on the use of varying the constant quantity in summing up equations with variable coefficients, by Dr. Brunacci.

2. A systematical enumeration of the coleoptera found in the territory of Saluzzo, with observations, by Law. Ponza. To this catalogue are annexed two plates, containing the following new species. *Coccinella numeralis,—c. obsoleta,—curculio spinosus,—c. dubius,—c. rugosus,—cerambyx præustus,—c. melanocephalus,—chrysomela melanocephala,—ch. variegata,—ch. pretiosa,—ch. luctuosa,—scarabæus rufescens,—cantharis impressifrons,—attelabus funereus,—dytiscus silphoides,—tenebrio rufus,—birrhus rossii,—carabus attenuatus,—c. metallicus,—c. rossii,—forficula bipunctata,—silpha sinuata,—s. scabra.*

3. On the motion of the hairs of the *hypnum adiantoides*, by Palamedas de Suffren. Parts endued with irritability had already been observed in the hairs of some mosses. Mr. De S. has found it in those of the *h. a.* and describes all the singularities of the phenomenon. This paper is accompanied with a plate.

4. Of a resin employed by the bee in constructing its combs. By Fr. Mouxy Deloche.

5. Entomological observations; by Mr. Disderi. Mr. D. first sketches the history of the silkworm; and then proceeds to certain hymenoptera, chiefly of the *genera tenthredo, ichneumon, sphex, et vespa.*

6. Specimen of the fungi of the vale of Pisa, by Hugh Camino. The new species are figured on three plates. They are Agaricus elatior: a. miniatus: a. pezizoides: a. astrosanguineus: a. tricolor: Boletus scobinaceus: Helvella grandis: h. reflexa: h. inflata: Peziza achracea: p. pyriformis: Reticularia rosea: Mucor fruticulosus.

7. Observations on the native gold found among sand, by Lew. Bossi, of Milan.

To

Barometer.

MY correſpondent from Edinburgh is reminded, with regard to his project for a barometer, that no enlargement or diminution of the bore will make the leaſt difference in the ſcale of the common barometer, conſiſting of a tube or veſſel, cloſed above, and having its lower end open, and communicating with a baſon of mercury of conſiderable diameter.

Subdiviſion of an arc by wheel and chain.

The contrivance, received ſome time ago from T. I. for making an aſtronomical inſtrument, in which the angular quantities ſhall be meaſured by the communication of a chain, ſtrap, or ſtring, poſſeſſes ſo much ingenuity and promiſe, that it has exerciſed the heads and hands of a number of eminent men. Among theſe are Robert Hooke, for a quadrant; Muſchenbroeck, for a pyrometer, and many operative men, ſuch as Siſſon and others, for theodolites and quadrants.—Where the intention of the inſtrument is ſimply to magnify the motion, without any particular attention to preciſion, the contrivance has a happy effect; particularly in public lectures, where a number of ſpectators may obſerve the ſame effect at the ſame time. It is likewiſe cheap, and may be carried into effect in ſituations where the uſe and application of more accurate apparatus cannot be referred to.

It cannot be made very exact.

A ſlight attention to the ſubject, will ſhew that all contrivances of the kind here alluded to muſt be conſiderably inaccurate. For they demand, 1, that the wheels ſhould be very truly circular: 2, and free from all dirt and impurity: 3. that they be well centered: 4. that the chain or ſtring ſhould be every where of the ſame thickneſs: 5, and its tenſion in all poſitions alike, &c. &c. If the quantity of error, taken at a minimum, which muſt ariſe from theſe and other cauſes, be attended to, it will be found that a ſimple diviſion of an arc (ſubdivided by a ſcrew or a nonius) and examined or read off by a ſmall magnifier, will afford greater preciſion; even when the work is performed by a careful deſigner, who is no mathematical inſtrument maker. It is certain that much greater delicacy and preciſion may be had in the diviſion of mathematical inſtruments by the patient diligence of a cultivator of practical mechanics than is generally ſuppoſed.

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SUPPLEMENT TO VOL. XIII.

ARTICLE I.

On the Saline Efflorescences upon Walls; Salivary Concretions; Deflagration of Mercury by Galvanism; Biliary Calculi; and the freezing Point of Spermaceti. By John Bostock, *M. D.*

To Mr. NICHOLSON.

SIR,

In the third and sixth volumes of your Journal you have inserted an account of some experiments that I performed on the saline efflorescences found upon walls. I have lately had an opportunity of examining two other specimens, of which I now send you the particulars. The first was obtained in considerable quantity from the inner walls of a warehouse that had been erected about twenty years. By a series of simple experiments, which it is unnecessary to detail at full length, I found it to be a sulphate of soda, which, as in the former cases, seemed to exist in a state of almost perfect purity. The circumstances attending the second of these efflorescences were more singular. It was given me by a friend who had scraped it from off the stones which are situated on the inside of the west aisle of York Minster. My friend, on whose accuracy

Examination of two specimens of efflorescence found upon walls.

The first was sulphate of soda:

The second was scraped from the

face of the stone within side York Minster, and not from the mortar.

curacy I place the fullest confidence, expressly stated, that it was taken from the surface of the stone itself, and not from the joints, or any part that had been covered with mortar. It existed there in large quantity, and was disposed in the form of projecting spiculæ. Upon subjecting it to the usual trials, I found it to be a very pure sulphate of magnesia. In order to ascertain with precision the degree of its purity, I prepared a quantity of the sulphate of magnesia, by uniting together its constituent parts. This artificial salt, and the salt from York, after being crystallized, were exposed for some time to the same degree of heat, and when all the water of crystallization appeared to be expelled, equal weights of them were dissolved in equal weights of water: 100 grains of these solutions had the muriate of barytes respectively added, until no farther precipitation was produced, when it appeared that exactly the same weight of barytes was necessary to saturate each solution. The portions of precipitated sulphate of barytes were collected and dried, and when examined by a nice balance, exhibited scarcely any perceptible difference in weight; they each amounted to 7.9 grains. A similar process being adopted with respect to the common Epsom salt of the shops, the precipitate was found to be 7.35 grains only. Before I quit this subject, I may remark that another friend, in visiting the cathedral at Tewkesbury, noticed a saline efflorescence on the inside of some part of that building; he collected a portion of it, intending to give it me for examination; but it was accidentally lost. Perhaps some of your readers, who reside in that neighbourhood, may be induced to examine it, and transmit the result to your Journal. I confess myself totally unable to explain the production of the sulphate of magnesia on the surface of a freestone, such as, I believe, forms the body of York Minster.

It was sulphate of magnesia, very pure.

Qu. Whence came the magnesia?

Account of a concretion in the salivary ducts.

Among the solid concretions which are formed in different parts of the human body, those from the salivary ducts are occasionally met with. I lately procured one of these substances, of which I will give you a brief account. It was a cylinder, pointed at one end, of half an inch in length, and somewhat more than $\frac{1}{12}$ of an inch in diameter; it weighed $1\frac{1}{2}$ gr. It was white and smooth on the outside, and its internal fracture did not exhibit any marks of regular organization. To half a grain of the concretion a few drops of diluted

muriatic

muriatic acid were added; no effervescence was excited. By the application of a gentle heat the whole was dissolved, except a few films that swam in the fluid. A copious precipitation was produced in this solution by pure ammonia, but none by the carbonate of ammonia. A part of the muriatic solution was evaporated; the residue was not soluble in water, but was speedily re-dissolved by the muriatic acid. The muriatic solution, saturated with the carbonate of ammoniac, had a precipitation produced by the oxalate of ammoniac. It appears therefore that the concretion consisted of the phosphate of lime, mixed with a little animal matter, probably coagulated albumen; it did not contain any carbonate of lime, and its component parts appeared not to possess any regularly organized structure. M. Fourcroy * and Dr. Thomson † have examined similar bodies, and agree in considering the earthy matter to be the phosphate of lime; we may therefore reasonably conclude that this substance always composes the earthy part of the salivary concretions. I am disposed, however, to differ from these distinguished chemists in my idea respecting the nature of the animal matter which enters into their composition; M. Fourcroy considers it as consisting of a species of mucilage, while Dr. Thomson describes it as "a membranous substance, which retains the shape of the concretion after the solution of the phosphate." This was certainly not the case with the one which I examined. I am disposed to consider the animal matter as coagulated albumen, rather than mucus, in consequence of its insoluble nature, and the greater facility with which it would on this account be detained by the phosphate of lime.

It was phosphate of lime chiefly, —with coagulated albumen.

The power which the electric fluid possesses, when generated by the galvanic apparatus, of burning metallic plates, affords one of the most beautiful experiments of which the science of chemistry can boast. All the metals have by this means been subjected to combustion, except mercury, which, owing to its fluidity, is incapable of being formed into thin laminæ. ‡ I have, however, been fortunate enough to accomplish this object, and that by the most simple method.

Mercury has not heretofore been deflagrated by galvanism.

* Systeme, IX. 368. † Chemistry, IV. 658.
‡ Thomson's Chemistry, I. 125.

 I was

I was performing some experiments with Mr. Richard Dalton, an ingenious lecturer in natural philosophy of this place, with a pile composed of 60 pair of six-inch plates of zinc and copper, when it occurred to me to place a minute globule of mercury in an iron spoon, resting on the top of the pile, and to approach to it a thick iron wire connected with the other end of the apparatus; the effect was, that a brilliant star of light was produced from the mercury, attended with a crackling noise and a copious emission of sparks; the mercury was found converted into the black oxide.

Experiment in which this was effected.

The dark coloured particles of gall-stones do not appear to be inspissated resin of the bile.

The most common species of biliary calculus is that composed of the peculiar crystalline matter, which in some of its properties resembles spermaceti, through which are interspersed a number of dark coloured particles, that are supposed to consist of hardened bile. This is the idea entertained by M. Fourcroy,* and the one which I adopted, when I made the experiments on this subject which are related in the fourth volume of your Journal. I have, however, since that time been disposed to alter my opinion; in two specimens of the biliary calculi, which I examined, after separating the crystalline matter by alcohol, I was unable to dissolve the dark coloured particles by any menstruum which I applied to them; they imparted a yellowish tinge to water and other fluids, but the great bulk of their substance remained unchanged. It is, I conceive, not probable that the mere inspissation of the resin of the bile could so far alter its properties. I mention this circumstance principally with a view of attracting the attention of any of your readers who may be in possession of a number of gall-stones, so as to ascertain whether the untractable nature of these particles is a general property of the cystic-adipobilious concretions, or something peculiar to the specimens upon which I experimented.

Melting point of spermaceti. Repetition of experiment confirms that it is a little above 112°.

I shall conclude this miscellaneous letter with some remarks upon the melting point of spermaceti. In the paper to which I have already referred, I mentioned the diversity of opinion that had been entertained on this subject, and afterwards stated that my own experience induced me to fix it at the 112th degree. Dr. Thomson, in the first volume of his Chemistry, fixes the melting point at 133°,† while in the fourth he states

* Systême, X. 59.   † Page:

it to be 112°, upon the authority of my paper.* Yet in his anſwer to the Edinburgh reviewers, he has mentioned this eſtimate of the melting point of ſpermaceti as one of his acknowledged errors, and upon the authority of Dr. Gibbes, fixes it at 115°. This circumſtance determined me to repeat the experiment; I employed a very delicate thermometer, and uſed every requiſite precaution; the reſult was that the inſtrument deſcended to *a little above the* 112*th degree*, and remained ſtationary until the ſubſtance was become ſolid. I may add that Dr. Irvine, in ſome experiments related in the ninth vol. of your Journal, fixes the point at 113°, which agrees ſo nearly with my obſervations, as to afford me an additional confidence in their accuracy.

I am, Sir,

Your obedient ſervant,

JOHN BOSTOCK.

Liverpool, April 9, 1806.

II.

Inveſtigation of the Temperature at which Water is of greateſt Denſity, from the Experiments of Dr. Hope on the Contraction of Water by Heat at low Temperatures. In a Letter from Mr. JOHN DALTON.

To Mr. NICHOLSON.

SIR,

IN your Journal for February 1805 was inſerted a letter of mine containing certain facts relative to the ſubject of my preſent communication, which led me to diſbelieve the common opinion that water is denſeſt at 40°, and inclined me to think it is at 32°. Since that time my attention has again been turned to the ſubject; ſome ſmall but immaterial corrections of the facts have been made and additional ones obtained, by which I have been enabled to demonſtrate, at leaſt to my own ſatisfaction, that the temperature at which water is of greateſt denſity is at or near 36° of Fahrenheit. The reſults have lately been communicated to the Mancheſter Society, and may

Reference to a former communication, in which the maximum denſity of water was taken at 32°.

Preſent inference that it is a 36°.

* Page 512.

perhaps

perhaps appear in a future volume. My prefent object is to fhew that the refults of Dr. Hope's experiments are explicable on the fuppofition of water being denfeft at 36°, but on no other.

Obfervations on the expanfions of water on each fide of its point of greateft denfity.

Dr. Hope and myfelf concur in the opinion that water is denfeft at fome one point of temperature, and that above and below that point it expands alike by heat and cold in a gradually increafing manner. De Luc was the firft to obferve that the expanfion is the fame quantity for the fame number of degrees, whether of increafe or diminution of temperature; the remarkable fact was extended by my former experience from a range of 8° to 25° or more, above and below the ftationary point. I have lately examined this fact with greater attention to precifion than formerly, and find that it is accurate, except that the expanfion for degrees *below* the ftationary point is always fomewhat more than for a correfponding number of degrees *above* the faid point. Thus, water is ftationary in a glafs thermometer at 42°; if heated to 75° by the mercurial fcale, it expands very confiderably; if plunged into a frigorific mixture of 13°, it falls to 12°, and then expands again to the fame point of 75°, at which it remains ftationary as long as continued in the mixture. It may be remarked too, that congelation rarely if ever takes place in the bulb, when the mixture is not below 15°, which may eafily be procured by putting fnow into water faturated with common falt. Hence we fee that 29° below, afford the fame expanfion as 33° above the ftationary point. This, I imagine, is occafioned by the error attached to the equal divifion of the mercurial fcale. For a fmall number of degrees, however, we may admit that the expanfions for correfponding intervals above and below are equal; hence we obtain the following table of correfponding temperatures at which water is of the fame denfity.

Suppofing greateft denfity at 40°				at 36°	
	39° and	41°		35° and	37°
	38 ——	42		34 ——	38
Correfponding denfities will be at	37 ——	43	Correfponding denfities will be at	33 ——	39
	36 ——	44		32 ——	40
	35	45		31 ——	41
	34	46		30 ——	42
	33	47		29 ·	
	32	48		28 ·	44

Dr.

The author and Dr. Hope concur that water may continue fluid below its freezing point.

Dr. Hope also admits with me the fact that water subjected to be cooled without agitation in a frigorific mixture, usually descends several degrees below the freezing point, and still retains its liquidity. Though it is easy to obtain water in a glass bulb 20 or 25° below freezing, I could never cool water in an open jar more than 10 or 11° below freezing, agreeable to the experience of Sir Charles Blagden. But I find water in such circumstances will admit of being cooled to 25°, and the bulb of a thermometer to be immersed and withdrawn several times, without freezing.

We come now to the experiments of Dr. Hope.

Experiment I.

Dr. Hope's experiment quoted.

A jar eight inches deep and 4½ in diameter, filled with water of 32°, and placed on a table, &c. Air 60—62°. Two thermometers inserted, one at the top, another at the bottom.

	Top Thermometer.	Differences.	*Bottom Thermometer.*	Differences.
	32°	0	32°	
		1+		2+
In 10 min.	33+		34+	
		2.5—		3—
30 ——	35.5		37	
		1.5		1+
50 ——	37		38+	
		1		0
1 hour ——	38		38+	
		4		.25—
1 10 ——	42		38.25	
		2		1.75
1 30 ——	44		40	&c.

Inference that the water descends as it acquires heat.

In the first interval of 10 minutes we observe the bottom thermometer to have gained 2°+, and the top only 1°+; the former has the heat which enters directly, together with the heat which descends by the side of the vessel; the latter has only the heat which enters directly, and as these are nearly as one to two, we may infer that the acquisition of direct heat, and heat by descent, are nearly equal in the bottom thermometer during that interval.

In the next interval of 20 minutes we observe the bottom thermometer gains 3°—, and the top 2°,5—. Here we see the

the ascending current still continues, but has produced little effect, having not added more than half a degree to the temperature.

—until the temperature rather exceeds 36°.

During the next 20 minutes the top gains 1°.5, the bottom only 1°+. In this interval we may observe the current has turned, but not yet acquired much force. The point of greatest density must therefore have existed at the last observation or near it: the mean of 35°.5 and 37° is 36¼° for the required point, as deduced from this experiment.

After which the heated water ascends.

In 10 minutes more the top gains 1°, and the bottom little or nothing; here we find the ascending current has become such as to manifest its influence very sensibly.

In the next 10 minutes the top gains 4°, and the bottom only ,25; here the ascending current has become quadruple what it was 2° below; because the farther the temperature is raised above the stationary point, the more powerful is the force of ascent arising from the same interval of temperature.

These facts do not agree with the supposed maximum density at 39° or 40°.

It would be in vain to attempt to reconcile the above experiment with the opinion that water is densest at 39° or 40°. At the very moment when the mean temperature of the water is 39°, we observe the *ascending* current the most active, when it ought to have been *descending* or imperceptible.

The effect is not modified by the table or support.

I once imagined that the experiment might be explained on the supposition of 32° being the point of greatest density; that the sudden increase of temperature at the bottom arose from the heat of the table, and that the cohesion of the particles of water prevented their ascent under the propulsion of so small a force; but having procured a large glass jar which could be suspended, I found the same order of differences nearly as when placed on a table, and was therefore obliged to abandon that explanation.

Intending to send the remainder of this investigation for a future number, I remain

Your friend,

JOHN DALTON.

Manchester, April 14, 1806.

III.

Account of a Series of Experiments, shewing the Effects of Compression in modifying the Action of Heat. By Sir James Hall, *Bart. F. R. S. Edinburgh.*

(Concluded from Page 328.)

Section III.

Experiments made in Tubes of Porcelain.—Tubes of Wedgwood's Ware.—Methods used to confine the carbonic Acid, and to close the Pores of the Porcelain in a horizontal Apparatus.—Tubes made with a View to these Experiments.—The vertical Apparatus adopted.—View of Results obtained, both in Iron and Porcelain.—The Formation of Lime-stone and Marble.—Inquiry into the Cause of the partial Calcinations.—Tubes of Porcelain weighed previous to breaking—Experiments with Porcelain Tubes proved to be limited.

WHILE I was carrying on the above-mentioned experiments, I was occasionally occupied with another set, in tubes of porcelain. So much, indeed, was I prepossessed in favour of this last mode, that I laid gun-barrels aside, and adhered to it during more than a year. The methods followed with this substance differ widely from those already described, though founded on the same general principles. Set of experiments in tubes of porcelain.

I procured from Mr. Wedgwood's manufactory at Etruria, in Staffordshire, a set of tubes for this purpose, formed of the same substance with the white mortars, in common use, made there. These tubes were fourteen inches long, with a bore of half an inch diameter, and thickness of 0.2; being closed at one end (figs. 9, 10, 11, 12, 13.) *Pl.* XI.

I proposed to ram the carbonate of lime into the breech (*Fig.* 9. A); then filling the tube to within a small distance of its muzzle with pounded flint (B), to fill that remainder (C) with common borax of the shops (borat of soda) previously reduced to glass, and then pounded; to apply heat to the muzzle alone, so as to convert that borax into solid glass; then, reversing the operation, to keep the muzzle cold, and apply the requisite heat to the carbonate lodged in the breech. They were closed at one end and the other aperture was stopped with glass of borax.

I thus expected to confine the carbonic acid; but the attempt was attended with considerable difficulty, and has led to Difficulties of this process.

to the employment of various devices, which I ſhall now ſhortly enumerate, as they occurred in the courſe of practice. The ſimple application of the principle was found inſufficient, from two cauſes: Firſt, The carbonic acid being driven from the breech of the tube, towards the muzzle, among the pores of the pounded ſilex, eſcaped from the compreſſing force, by lodging itſelf in cavities which were comparatively cold: Secondly, The glaſs of borax, on cooling, was always found to crack very much, ſo that its tightneſs could not be depended on.

And the method of partly obviating them.

To obviate both these inconveniences at once, it occurred to me, in addition to the firſt arrangement, to place ſome borax (*Fig.* 10. C) ſo near the breech of the tube, as to undergo heat along with the carbonate (A); but interpoſing between this borax and the carbonate, a ſtratum of ſilex (B), in order to prevent contamination. I truſted that the borax in a liquid or viſcid ſtate, being thruſt outwards by the expanſion of the carbonic acid, would preſs againſt the ſilex beyond it (D), and totally prevent the elaſtic ſubſtances from eſcaping out of the tube, or even from wandering into its cold parts.

In ſome reſpects, this plan anſwered to expectation. The glaſs of borax, which can never be obtained when cold, without innumerable cracks, unites into one continued viſcid maſs in the loweſt red-heat; and as the ſtreſs in theſe experiments begins only with redneſs, the borax being heated at the ſame time with the carbonate, becomes united and impervious, as ſoon as its action is neceſſary. Many good reſults were accordingly obtained in this way. But I found, in practice, that as the heat roſe, the borax began to enter into too thin fuſion, and was often loſt among the pores of the ſilex, the ſpace in which it had lain being found empty on breaking the tube. It was therefore found neceſſary to oppoſe ſomething more ſubſtantial and compact, to the thin and penetrating quality of pure borax.

Bottle glaſs was found much preferable to pure borax for the purpoſe of reſtraining the carbonic acid.

In ſearching for ſome ſuch ſubſtance, a curious property of bottle-glaſs occurred accidentally. Some of this glaſs, in powder, having been introduced into a muffle at the temperature of about 20° of Wedgwood; the powder, in the ſpace of about a minute, entered into a ſtate of viſcid agglutination, like that of honey, and in about a minute more, (the heat always continuing unchanged), conſolidated into a firm and compact

pact

pact mass of *Reaumur's porcelain* *. It now appeared, that by placing this substance immediately behind the borax, the penetrating quality of this salt might be effectually restrained; for, Reaumur's porcelain has the double advantage of being refractory, and of not cracking by change of temperature. I found, however, that in the act of consolidation, the pounded bottle-glass shrunk, so as to leave an opening between its mass and the tube, through which the borax, and, along with it, the carbonic acid, was found to escape. But the object in view was obtained by means of a mixture of pounded bottle-glass, and pounded flint, in equal parts. This compound still agglutinates, not indeed into a mass so hard as Reaumur's porcelain, but sufficiently so for the purpose; and this being done without any sensible contraction, an effectual barrier was opposed to the borax; (this arrangement is shewn in *Fig.* 11.); and thus the method of closing the tubes was rendered so complete, as seldom to fail in practice †. A still further refinement upon this method was found to be of advantage. A second series of powders, like that already described, was introduced towards the muzzle, (as shewn in *Fig.* 12). During the first period of the experiment, this last-mentioned series was exposed to heat, with all the outward half of the tube *(a b)*; and by this means, a solid mass was produced, which remained cold and firm during the subsequent action of heat upon the carbonate.

Improvements on this method.

I soon found, that notwithstanding all the above-mentioned precautions, the carbonic acid made its escape, and that it pervaded the substance of the Wedgwood tubes, where no flaw could be traced. It occurred to me, that this defect might be remedied, were borax, in its thin and penetrating state of fusion, applied to the inside of the tube; and that the pores of the porcelain might thus be closed, as those of leather are closed by oil, in an air-pump. In this view, I rammed the carbonate into a small tube, and surrounded it with pounded glass of borax, which, as soon as the heat was applied, spread on the in-

Remedy for porosity in the earthen tubes.

* In the same temperature, a mass of the glass of equal bulk would undergo the same change; but it would occupy an hour.

† A substance equally efficacious in restraining the penetrating quality of borax, was discovered by another accident. It consists of a mixture of borax and common sand, by which a substance is formed, which, in heat, assumes the state of a very tough paste, and becomes hard and compact on cooling.

side

fide of the large tube, and effectually clofed its pores. In this manner, many good experiments were made with barrels lying horizontally in common muffles, (the arrangement juft defcribed being reprefented in Fig. 13.)

Beft material for tubes.

I was thus enabled to carry on experiments with this porcelain, to the utmoft that its ftrength would bear. But I was not fatisfied with the force fo exerted; and hoping to obtain tubes of a fuperior quality, I fpent much time in experiments with various porcelain compofitions. In this, I fo far fucceeded, as to produce tubes by which the carbonic acid was in a great meafure retained without any internal glaze. The beft material I found for this purpofe, was the pure porcelain-clay of Cornwall, or a compofition in the proportion of two of this clay to one of what the potters call *Cornifh-ftone*, which I believe to be a granite in a ftate of decompofition. Thefe tubes were feven or eight inches long, with a bore tapering from 1 inch to 0.6. Their thicknefs was about 0.3 at the breech, and tapered towards the muzzle to the thinnefs of a wafer.

Improvement by placing the tube vertically.

I now adopted a new mode of operation, placing the tube vertically, and not horizontally, as before. By obferving the thin ftate of borax whilft in fufion, I was convinced, that it ought to be treated as a complete liquid, which being fupported in the courfe of the experiment from below, would fecure perfect tightnefs, and obviate the failure which often happened in the horizontal pofition, from the falling of the borax to the lower fide.

Particular defcription of the procefs.

In this view, (fig. 16.) I filled the breech in the manner defcribed above, and introduced into the muzzle fome borax (C) fupported at the middle of the tube by a quantity of filex mixed with the bottle glafs (B). I placed the tube, fo prepared, with its breech plunged into a crucible filled with fand (E), and its muzzle pointing upwards. It was now my object to apply heat to the muzzle-half, whilft the other remained cold. In that view, I conftructed a furnace (figs. 14 and 15.) having a muffle placed vertically (*c d*,) furrounded on all fides with fire (*e e*), and open both above (at *c*), and below (at *d*). The crucible juft mentioned, with its tube, being then placed on a fupport directly below the vertical muffle, (as reprefented in fig. 14. at F) it was raifed, fo that the half of the tube next the muzzle was introduced into

the

the fire. In confequence of this, the borax was feen from above to melt, and run down in the tube, the air contained in the powder efcaping in the form of bubbles, till at laft the borax ftood with a clear and fteady furface like that of water. Some of this falt being thrown in from above, by means of a tube of glafs, the liquid furface was raifed nearly to the muzzle, and, after all had been allowed to become cold, the pofition of the tube was reverfed; the muzzle being now plunged into the fand, (as in fig. 17.) and the breech introduced into the muffle. In feveral experiments, I found it anfwer well, to occupy great part of the fpace next the muzzle, with a rod of fand and clay previoufly baked, (fig. 19. K K), which was either introduced at firft, along with the pounded borax, or, being made red hot, was plunged into it when in a liquid ftate. In many cafes I affifted the compactnefs of the tube by means of an internal glaze of borax; the carbonate being placed in a fmall tube, (as fhewn in fig. 18.)

These devices anfwered the end propofed. Three-fourths [illegible] the tube next the muzzle was found completely filled with mafs, having a concave termination at both ends, (*f* and *g* [illegible] 17, 18, 19.) fhewing that it had ftood as a liquid in the [illegible] oppofite pofitions in which heat had been applied to it. [illegible] a degree of tightnefs indeed was obtained in this [illegible] I found myfelf fubjected to an unforefeen fource of [illegible] number of the tubes failed, not by explofion, but [illegible] formation of a minute longitudinal fiffure at the breech, through which the borax and carbonic acid efcaped. I faw that this arofe from the expanfion of the borax when in a liquid ftate, as happened with the fufible metal in the experiments with iron-barrels; for, the crevice here formed, indicated the exertion of fome force acting very powerfully, and to a very fmall diftance. Accordingly, this fource of failure was remedied by the introduction of a very fmall air-tube. This, however, was ufed only in a few experiments.

Effect of expanfion in the fufed borax upon the tubes.

In the courfe of the years 1801, 1802, and 1803, I made a number of experiments, by the various methods above defcribed, amounting, together with thofe made in gun barrels, to one hundred and fifty-fix. In an operation fo new, and in which the apparatus was ftrained to the utmoft of its power, conftant fuccefs could not be expected, and in fact many experiments failed, wholly or partially. The refults, however, upon

These experiments were upon the whole fuccefsful.

upon the whole, were ſatisfactory, ſince they ſeemed to eſtabliſh ſome of the eſſential points of this inquiry.

Theſe experiments prove, that, by mechanical conſtraint, the carbonate of lime can be made to undergo ſtrong heat, without calcination, and to retain almoſt the whole of its carbonic acid, which, in an open fire, at the ſame temperature, would have been entirely driven off: and that, in theſe circumſtances, heat produces ſome of the identical effects aſcribed to it in the Huttonian Theory.

Pounded carbonate of lime in its ſeveral varieties became agglutinated into ſtony maſſes.

By this joint action of heat and preſſure, the carbonate of lime which had been introduced in the ſtate of the fineſt powder, is agglutinated into a firm maſs, poſſeſſing a degree of hardneſs, compactneſs, and ſpecific gravity * nearly approaching to theſe qualities in a ſound limeſtone; and ſome of the reſults, by their ſaline fracture, by their ſemitranſparency, and their ſuſceptibility of poliſh, deſerve the name of marble.

The ſame trials have been made with all calcareous ſubſtances; with chalk, common limeſtone, marble, ſpar, and the ſhells of fiſh. All have ſhewn the ſame general property, with ſome varieties as to temperature. Thus, I found, that, in the ſame circumſtances, chalk was more ſuſceptible of agglutination than ſpar; the latter requiring a heat two degrees higher than the former, to bring it to the ſame pitch of agglutination.

The chalk uſed in my firſt experiments, always aſſumed the character of a yellow marble, owing probably to ſome ſlight contamination of iron. When a ſolid piece of chalk, whoſe bulk had been previouſly meaſured in the gauge of Wedgwood's pyrometer was ſubmitted to heat under compreſſion, its contraction was remarkable, proving the approach of the particles during their conſolidation; on theſe occaſions, it was found to ſhrink three times more than the pyrometer-pieces in the ſame temperature. It loſt, too, almoſt entirely, its power of imbibing water, and acquired a great additional ſpecific gravity. On ſeveral occaſions, I obſerved, that maſſes of chalk, which, before the experiment, had ſhewn one uniform character of whiteneſs, aſſumed a ſtratified appearance, indicated by a ſeries of parallel layers of a brown colour. This

* See Appendix.

circumſtance

circumſtance may hereafter throw light on the geological hiſtory of this extraordinary ſubſtance.

I have ſaid, that, by mechanical conſtraint, almoſt the whole of the carbonic acid was retained. And, in truth, at this period, ſome loſs of weight had been experienced in all the experiments, both with iron and porcelain. But even this circumſtance is valuable, by exhibiting the influence of the carbonic acid, as varied by its quantity.

And moſt of the carbonic acid was retained.

When the loſs exceeded 10 or 15 *per cent.* * of the weight of the carbonate, the reſult was always of a friable texture, and without any ſtony character; when leſs than two or three *per cent.* it was conſidered as good, and poſſeſſed the properties of a natural carbonate. In the intermediate caſes, when the loſs amounted, for inſtance, to ſix or eight *per cent.* the reſult was ſometimes excellent at firſt, the ſubſtance bearing every appearance of ſoundneſs, and often poſſeſſing a high character of cryſtallization; but it was unable to reſiſt the action of the air; and, by attracting carbonic acid or moiſture, or both, crumbled to duſt more or leſs rapidly, according to circumſtances. This ſeems to prove, that the carbonate of lime, though not fully ſaturated with carbonic acid, may poſſeſs the properties of limeſtone; and perhaps a difference of this kind may exiſt among natural carbonates, and give riſe to their different degrees of durability.

Qualities of the product differing according to the loſs of carbonic acid.

I have obſerved, in many caſes, that the calcination has reached only to a certain depth into the maſs; the internal part remaining in a ſtate of complete carbonate, and, in general, of a very fine quality. The partial calcination ſeems thus to take place in two different modes. By one, a ſmall proportion of carbonic acid is taken from each particle of carbonate; by the other, a portion of the carbonate is quite calcined, while the reſt is left entire. Perhaps one reſult is the effect of a feeble calcining cauſe, acting during a long time, and the other of a ſtrong cauſe, acting for a ſhort time.

Some of the reſults which ſeemed the moſt perfect when firſt produced, have been ſubject to decay, owing to partial calcination. It happened, in ſome degree, to the beautiful

Some reſults were ſubject to decay from partial calcination.

* I have found, that, in open fire, the entire loſs ſuſtained by the carbonate varies in different kinds from 42 to 45.5 *per cent.*

ſpecimen

ſpecimen produced on the 3d of March, 1801, though a freſh fracture has reſtored it.

A ſpecimen, too, of marble, formed from pounded ſpar, on the 15th of May, 1801, was ſo complete as to deceive the workman employed to poliſh it, who declared, that were the ſubſtance a little whiter, the quarry from which it was taken would be of great value, if it lay within reach of a market. Yet, in a few weeks after its formation, it fell to duſt.

Very many were durable marble.

Numberleſs ſpecimens, however, have been obtained, which reſiſt the air, and retain their poliſh as well as any marble. Some of them continue in a perfect ſtate, though they have been kept without any precaution during four or five years. That ſet, in particular, remain perfectly entire, which were ſhewn laſt year in this Society, though ſome of them were made in 1799, ſome in 1801 and 1802, and though the firſt eleven were long ſoaked in water, in the trials made of their ſpecific gravity.

Remarkable fact.

A curious circumſtance occurred in one of theſe experiments, which may hereafter lead to important conſequences. Some ruſt of iron had accidentally found its way into the tube: 10 grains of carbonate were uſed, and a heat of 28° was applied. The tube had no flaw; but there was a certainty that the carbonic acid had eſcaped through its pores. When broken, the place of the carbonate was found occupied, partly by a black ſlaggy matter, and partly by ſphericles of various ſizes, from that of a ſmall pea downwards, of a white ſubſtance, which proved to be quicklime; the ſphericles being interſperſed through the ſlag, as ſpar and agates appear in whinſtone. The ſlag had certainly been produced by a mixture of the iron with the ſubſtance of the tube; and the ſpherical form of the quicklime ſeems to ſhew, that the carbonate had been in fuſion along with the ſlag, and that they had ſeparated on the eſcape of the carbonic acid.

The ſubject was carried thus far in 1803, when I ſhould probably have publiſhed my experiments, had I not been induced to proſecute the inquiry by certain indications, and accidental reſults, of a nature too irregular and uncertain to meet the public eye, but which convinced me, that it was poſſible to eſtabliſh by experiment the truth of all that was hypothetically aſſumed in the Huttonian theory.

Endeavour to improve the experiments by

The principal object was now to accompliſh the entire fuſion of the carbonate, and to obtain ſpar as the reſult of that

fuſion

fufion, in imitation of what we conceive to have taken place in nature.

It was likewife important to acquire the power of retaining all the carbonic acid of the carbonate, both on account of the fact itfelf, and on account of its confequences; the refult being vifibly improved by every approach towards complete faturation. I therefore became anxious to inveftigate the caufe of the partial calcinations which had always taken place, to a greater or lefs degree, in all thefe experiments. The queftion naturally fuggefts itfelf, What has become of the carbonic acid, feparated in thefe partial calcinations from the earthy bafis? Has it penetrated the veffel, and efcaped entirely, or has it been retained within it in a gafeous, but highly compreffed ftate? It occurred to me, that this queftion might be eafily refolved, by weighing the veffel before and after the action of heat upon the carbonate.

preventing all calcination, and perfectly fufing the carbonate.

With iron, a conftant and inappreciable fource of irregularity exifted in the oxidation of the barrel. But with porcelain the thing was eafy; and I put it in practice in all my experiments with this material, which were made after the queftion had occurred to me. The tube was weighed as foon as its muzzle was clofed, and again, after the breech had been expofed to the fire; taking care, in both cafes, to allow all to cool. In every cafe, I found fome lofs of weight, proving, that even in the beft experiments, the tubes were penetrated to a certain degree. I next wifhed to try if any of the carbonic acid feparated, remained within the tube in a gafeous form; and in that view, I wrapt the tube, which had juft been weighed, in a fheet of paper, and placed it, fo furrounded, on the fcale of the balance. As foon as its weight was afcertained, I broke the tube by a fmart blow, and then replaced upon the fcale the paper containing all the fragments. In thofe experiments, in which entire calcination had taken place, the weight was found not to be changed, for all the carbonic acid had already efcaped during the action of heat. But in the good refults, I always found that a lofs of weight was the confequence of breaking the tube.

By experiment it was found that iron barrels vitiated the refult.

Thefe facts prove, that both caufes of calcination had operated in the porcelain tubes; that, in the cafes of fmall lofs, part of the carbonic acid had efcaped through the veffel, and that part had been retained within it. I had in view methods by

With porcelain tubes this caufe of irregularity exifted along with the efcape of the acid

by which the laſt could be counteracted; bnt I ſaw no remedy for the firſt. I began, therefore, to deſpair of ultimate ſucceſs with tubes of porcelain *.

These laſt could not bear elevated heats.

Another circumſtance confirmed me in this opinion. I found it impracticable to apply a heat above 27° to theſe tubes, when charged as above with carbonate, without deſtroying them, either by exploſion, by the formation of a minute rent, or by the actual ſwelling of the tube. Sometimes this ſwelling took place to the amount of doubling the internal diameter, and yet the porcelain held tight, the carbonate ſuſtaining but a very ſmall loſs. This ductility of the porcelain in a low heat is a curious fact, and ſhews what a range of temperature is embraced by the gradual tranſition of ſome ſubſtances from a ſolid to a liquid ſtate: For the ſame porcelain, which is thus ſuſceptible of being ſtretched out without breaking in a heat of 27°, ſtands the heat of 152°, without injury, when expoſed to no violence, the angles of its fracture remaining ſharp and entire.

IV.

Experiments in Gun-Barrels reſumed.—The Vertical Apparatus applied to them.—Barrels bored in ſolid Bars.—Old Sable Iron.—Fuſion of the Carbonate of Lime.—Its Action on Porcelain.—Additional Apparatus required in Conſequence of that Action.—Good Reſults; in particular, four Experiments, illuſtrating the Theory of Internal Calcination, *and ſhewing the Efficacy of the Carbonic Acid as a Flux.*

Experiments with gun barrels reſumed,

SINCE I found that, with porcelain tubes, I could neither confine the carbonic acid entirely, nor expoſe the carbonate in them to ſtrong heats; I at laſt determined to lay them aſide, and return to barrels of iron, with which I had formerly obtained ſome good reſults, favoured, perhaps, by ſome accidental circumſtances.

* I am nevertheleſs of opinion, that, in ſome ſituations, experiments with compreſſion may be carried on with great eaſe and advantage in ſuch tubes. I allude to the ſituation of the geologiſts of France and Germany, who may eaſily procure, from their own manufactories, tubes of a quality far ſuperior to any thing made for ſale in this country.

On

On the 12th of February, 1803, I began a series of experiments with gun-barrels, resuming my former method of working with the fusible metal, and with lead; but altering the position of the barrel from horizontal to vertical; the breech being placed upwards, bring the action of heat on the carbonate. This very simple improvement has been productive of advantages no less remarkable, than in the case of the tubes of porcelain. In this new position, the included air, quitting the air-tube on the fusion of the metal, and rising to the breech, is exposed to the greatest heat of the furnace, and must therefore react with its greatest force; whereas, in the horizontal position, that air might go as far back as the fusion of the metal reached, where its elasticity would be much feebler. The same disposition enabled me to keep the muzzle of the barrel plunged, during the action of heat, in a vessel filled with water; which contributed very much both to the convenience and safety of these experiments.

In a vertical position, with the breech upwards, and stopped with fusible metal.

In this view, making use of the brick-furnace with the vertical muffle, already described in page 384, I ordered a pit (*a a a fig.* 20.) to be excavated under it, for the purpose of receiving a water-vessel. This vessel (represented separately, *fig.* 21.) was made of cast iron; it was three inches in diameter, and three feet deep; and had a pipe (*d e*) striking off from it at right angles, four or five inches below its rim, communicating with a cup (*e f*) at the distance of about two feet. The main vessel being placed in the pit (*a a*) directly below the vertical muffle, and the cup standing clear of the furnace, water poured into the cup flowed into the vessel, and could thus conveniently be made to stand at any level. (The whole arrangement is represented in *fig.* 20.) The muzzle of the barrel (g) being plunged into the water, and its breech (*b*) reaching up into the muffle, as far as was found [illegible], its position was secured by an iron chain (*g f*). [illegible] ated downwards generally kept the surface [illegible] in a state of ebullition; the waste thus occas[illegible] supplied by means of the cup, into which, if necessar[illegible] constant stream could be made to flow.

Description and drawing of the apparatus.

As form[illegible] I rammed the carbonate into a tube of porcelain, and placed it in a cradle of iron, along with an air-tube and a pyrometer; the cradle being fixed to a rod of iron, which rod I now judged proper to make as large as the barrel

would

would admit, in order to exclude as much of the fusible metal as poſſible; for the expanſion of the liquid metal being in proportion to the quantity heated, the more that quantity could be reduced, the leſs riſk there was of deſtroying the barrels.

Simple mode of withdrawing the contents from the tube.

In the courſe of practice, a ſimple mode occurred of removing the metal and withdrawing the cradle: it conſiſted in placing the barrel with its muzzle downwards, ſo as to keep the breech above the furnace and cold, while its muzzle was expoſed to ſtrong heat in the muffle. In this manner, the metal was diſcharged from the muzzle, and the poſition of the barrel being lowered by degrees, the whole metal was removed in ſucceſſion, till at laſt the cradle and its contents became entirely looſe. As the metal was delivered, it was received in a crucible, filled with water, ſtanding on a plate of iron placed over the pit, which had been uſed, during the firſt ſtage of the experiment, to contain the water-veſſel. It was found to be of ſervice, eſpecially where lead was uſed, to give much more heat to the muzzle than ſimply what was required to liquefy the metal it contained; for when this was not done, the muzzle growing cold as the breech was heating, ſome of the metal delivered from the breech was congealed at the muzzle, ſo as to ſtop the paſſage.

According to this method, many experiments were made in gun-barrels, by which ſome very material ſteps were gained in the inveſtigation.

Experiment in the new method.

On the 24th of February, I made an experiment with ſpar and chalk; the ſpar being placed neareſt to the breech of the barrel, and expoſed to the greateſt heat, ſome baked clay intervening between the carbonates. On opening the barrel, a long-continued hiſſing noiſe was heard. The ſpar was in a ſtate of entire calcination; the chalk, though crumbling at the outſide, was uncommonly hard and firm in the heart. The temperature had riſen to 32°.

Internal calcination, where the carbonic acid did not eſcape out of the apparatus.

In this experiment, we have the firſt clear example, in iron barrels, of what I call *Internal Calcination*; that is to ſay, where the carbonic acid ſeparated from the earthy baſis, has been accumulated in cavities within the barrel. For, ſubſequently to the action of ſtrong heat, the barrel had been completely cooled; the air therefore introduced by means of the air-tube, muſt have reſumed its original bulk, and by itſelf could

have

have no tendency to rush out; the heat employed to open the barrel ; barely sufficient to soften the metal. Since, the opening of the barrel was accompanied by the discharge of elastic matter in great abundance, it is evident, that this must have proceeded from something superadded to the air originally included, which could be nothing but the carbonic acid of the carbonate. It follows, that the calcination had been, in part at least, internal; the separation of the acid from the earthy matter being complete where the heat was strongest, and only partial where the intensity was less.

The chemical principles stated in a former part of this paper, authorised us to expect a result of this kind. As heat, by increasing the volatility of the acid, tended to separate it from the earth, we had reason to expect, that, under the same compression, but in different temperatures, one portion of the carbonate might be calcined, and another not: And that the least heated of the two, would be the least exposed to a change not only from want of heat, but likewise in consequence of the calcination of the other mass; for the carbonic acid disengaged by the calcination of the hottest of the two, must have added to the elasticity of the confined elastic fluid, so as to produce an increase of compression. By this means, the calcination of the coldest of the two might be altogether prevented, and that of the hottest might be hindered from making any further advancement. This reasoning seemed to explain the partial calcinations which had frequently occurred where there was no proof of leakage; and it opened some new practical views in these experiments, of which I availed myself without loss of time. If the internal calcination of one part of an inclosed mass, promotes the compression of other masses included along with it, I conceived that we might forward our views very much by placing a small quantity of carbonate, carefully weighed, in the same barrel with a large quantity of that substance; and by arranging matters so that the small fiducial part should undergo a moderate heat, while a stronger heat, capable of producing internal calcination, should be applied to the rest of the carbonate. In this manner, I made many experiments, and obtained results which seemed to confirm this reasoning, and which were often very satisfactory, though the heat did not always exert its greatest force where I intended it to do so.

Part of the included carbonate was calcined, another part retaining its acid. Reasoning on this fact.

On

Experiment. Carbonate partly fused, and in part deprived of carbonic acid.

On the 28th of February, I introduced some carbonate, accurately weighed, into a small porcelain tube, placed within a larger one, the rest of the large tube being filled with pounded chalk; these carbonates, together with some pieces of chalk, placed along with the large tube in the cradle, weighing in all 195.7 grains. On opening the barrel, air rushed out with a long-continued hissing noise. The contents of the little tube were lost by the intrusion of some borax which had been introduced over the silex, in order to exclude the fusible metal. But the rest of the carbonate, contained in the large tube, came out in a fine state, being porous and frothy throughout; sparkling every where with facetts, the angular form of which was distinguishable in some of the cavities by help of a lens; in some parts the substance exhibited the rounding of fusion; in many it was in a high degree transparent. It was yellow towards the lower end, and at the other almost colourless. At the upper end, the carbonate seemed to have united with the tube, and at the places of contact to have spread upon it; the union having the appearance of a mutual action. The general mass of carbonate effervesced in acid violently, but the thin stratum immediately contiguous to the tube, feebly, if at all.

Similar experiment, in which the carbonate exhibited more remarkable facettes.

On the 3d of March, I introduced into a very clean tube of porcelain 30.8 of chalk. The tube was placed in the upper part of the cradle, the remaining space being filled with two pieces of chalk, cut for the purpose; the uppermost of these being excavated, so as to answer the purpose of an air-tube. The pieces thus added, were computed to weigh about 300 grains. There was no pyrometer used; but the heat was guessed to be about 30°. After the barrel had stood during a few minutes in its delivering position, the whole lead with the rod and cradle, were thrown out with a smart report, and with considerable force. The lowermost piece of chalk had scarcely been acted upon by heat. The upper part of the other piece was in a state of marble, with some remarkable facettes. The carbonate, in the little tube, had shrunk very much during the first action of heat, and had begun to sink upon itself, by a further advancement towards liquefaction. The mass was divided into several cylinders, lying confusedly upon each other; this division arising from the manner in which the pounded chalk was rammed into the tube in successive portions. In several

feveral places, particularly at the top, the carbonate was very porous, and full of decided air-holes, which could not have been formed but in a foft fubftance; the globular form and fhining furface of all thefe cavities, clearly indicating fufion. The fubftance was femi-tranfparent; in fome places yellow, and in fome colourlefs. When broken, the folid parts fhewed a faline fracture, compofed of innumerable facettes. The carbonate adhered, from end to end, to the tube, and incorporated with it, fo as to render it impoffible to afcertain what lofs had been fuftained. In general, the line of contact was of a brown colour; yet there was no room for infpecting the prefence of any foreign matter, except, perhaps, from the iron-rod which was ufed in ramming down the chalk. But, in fubfequent experiments, I have obferved the fame brown or black colour at the union of the carbonate with the porcelain tubes, where the powder had been purpofely rammed with a piece of wood; fo that this colour, which has occurred in almoft every fimilar cafe, remains to be accounted for. The carbonate effervefced violently with acid; the fubftance in contact with the tube, doing fo, however, more feebly than in the heart, leaving a copious depofit of white fandy matter, which is doubtlefs a part of the tube, taken up by the carbonate in fufion.

On the 24th of March, I made a fimilar experiment, in a ftout gun-barrel, and took fome care, after the application of heat, to cool the barrel flowly, with a view to cryftallization. The whole mafs was found in a fine ftate, and untouched by the lead; having a femitranfparent and faline ftructure, with various facettes. In one part, I found the moft decided cryftallization I had obtained, though of a fmall fize: owing to its tranfparency it was not eafily vifible, till the light was made to reflect from the cryftalline furface, which then produced a dazzle, very obfervable by the naked eye; when examined by means of a lens, it was feen to be compofed of feveral plates, broken irregularly in the fracture of the fpecimen, all of which are parallel to each other, and reflect under the fame angle fo as to unite in producing the dazzle. This ftructure was obfervable equally well in both parts of the broken fpecimen. In a former experiment, as large a facette was obtained in a piece of folid chalk; but this refult was of more confequence, as having been produced from chalk previoufly pounded.

Another experiment with flow cooling. Saline ftructure and cryftallization produced in chalk previoufly pounded.

The

The gun-barrels, though superior to porcelain were still too weak.

The foregoing experiments proved the superior efficacy of iron vessels over those of porcelain, even where the thickness was not great; and I persevered in making a great many experiments with gun-barrels, by which I occasionally obtained very fine results: but I was at last convinced, that their thickness was not sufficient to ensure regular and steady success. For this purpose, it appeared proper to employ vessels of such strength, as to bear a greater expansive force than was just necessary; since, occasionally, (owing to our ignorance of the relation between the various forces of expansion, affinity, tenacity, &c.) much more strain has been given to the vessels than was requisite. In such cases, barrels have been destroyed, which, as the results have proved, had acted with sufficient strength during the first stages of the experiments, though they had been unable to resist the subsequent overstrain. Thus, my success with gun-barrels, depended on the good fortune of having used a force no more than sufficient, to constrain the carbonic acid, and enable it to act as a flux on the lime. I therefore determined to have recourse to iron barrels of much greater strength, and tried various modes of construction.

Barrels formed by boring in solid bars of iron which proved excellent.

I had some barrels executed by wrapping a thick plate of iron round a mandrel, as is practised in the formation of gun-barrels; and likewise by bringing the two flat sides together, so as to unite them by welding. These attempts, however, failed. I next thought of procuring bars of iron, and of having a cavity bored out of the solid, so as to form a barrel. In this manner I succeeded well. The first barrel I tried in this way was of small bore, only half an inch: Its performance was highly satisfactory, and such as to convince me, that the mode now adopted was the best of any that I had tried. Owing to the smallness of the bore, a pyrometer could not be used internally, but was placed upon the breech of the barrel, as it stood in the vertical muffle. In this position, it was evidently exposed to a much less heat than the fiducial part of the apparatus, which was always placed, as nearly as could be guessed, at the point of greatest heat.

Finely levigated spar became agglutinated by heat; semi-transparent, vitreous, with a few facettes.

On the 4th of April, an experiment was made in this way with some spar; the pyrometer on the breech giving 33°. The spar came out clean, and free from any contamination, adhering to the inside of the porcelain tube: it was very much shrunk

ſhrunk, ſtill retaining a cylindrical form, though bent by partial adheſions. Its ſurface bore ſcarcely any remains of the impreſſion taken by the powder, on ramming it into the tube: it had, to the naked eye, the roughneſs and ſemitranſparency of the pith of a ruſh ſtripped of its outer ſkin. By the lens, this ſame ſurface was ſeen to be glazed all over, though irregularly, ſhewing here and there ſome air-holes. In fracture, it was ſemitranſparent, more vitreous than cryſtalline, though having a few facettes: the maſs, was ſeemingly formed of a congeries of parts, in themſelves quite tranſparent: and, at the thin edges, ſmall pieces were viſible of perfect tranſparency. Theſe muſt have been produced in the fire; for the ſpar had been ground with water; and paſſed through ſieves, the ſame with the fineſt of thoſe uſed at Etruria, as deſcribed by Mr. Wedgwood, in his paper on the conſtruction of his pyrometer.

In theſe experiments the eſcape of carbonic acid appears to have been leſs.

With the ſame barrel I obtained many intereſting reſults, giving as ſtrong proofs of fuſion as in any former experiments; with this remarkable difference, that, in theſe laſt, the ſubſtance was compact, with little or no trace of frothing. In the gun-barrels where fuſion had taken place, there had always been a loſs of 4 or 5 *per cent.* connected, probably, with the frothing. In theſe experiments, for a reaſon ſoon to be ſtated, the circumſtance of weight could not be obſerved; but appearances led me to ſuppoſe, that here the loſs had been ſmall, if any.

Gradual failure of the barrel bored from the ſolid.

On the 6th of April, I made another experiment with the ſquare barrel, whoſe thickneſs was now much reduced by ſucceſſive ſcales, produced by oxidation, and in which a ſmall rent began to appear externally, which did not, however, penetrate to the bore. The heat roſe high, a pyrometer on the breech of the barrel giving 37°. On removing the metals, the cradle was found to be fixed, and was broken in the attempts made to withdraw it. The rent was much widened externally: but it was evident, that the barrel had not been laid open, for part of the carbonate was in a ſtate of ſaline marble; another was hard and white, without any ſaline grains, and ſcarcely effervefced in acid. It was probably quicklime, formed by internal calcination, but in a ſtate that has not occurred in any other experiment.

The

Remarkable fact of cryftals which appear to have been formed by fublimation

The workman whom I employed to take out the remains of the cradle, had cut off a piece from the breech of the barrel, three or four inches in length. As I was examining the crack which was feen in this piece, I was furprifed to fee the infide of the barrel lined with a fet of tranfparent and well-defined cryftals, of fmall fize, yet vifible by the naked eye. They lay together in fome places, fo as to cover the furface of the iron with a tranfparent coat; in others they were detached, and fcattered over the furface. Unfortunately, the quantity of this fubftance was too fmall to admit of much chemical examination; but I immediately afcertained, that it did not in the leaft effervefce in acid, nor did it feem to diffolve in it. The cryftals were in general tranfparent and colourlefs, though a few of them were tinged feemingly with iron. Their form was very well defined, being flat, with oblique angles, and bearing a ftrong refemblance to the cryftals of the Lamellated Stylbite of Hauy. Though made above two years ago, they ftill retain their form and tranfparency unchanged. Whatever this fubftance may be, its appearance, in this experiment, is in the higheft degree interefting, as it feems to afford an example of the mode in which Dr. Hutton fuppofes many internal cavities to have been lined, by the fublimation of fubftances in a ftate of vapour; or, held in folution, by matters in a gafeous form. For, as the cryftals adhered to a part of the barrel, which muft have been occupied by air during the action of heat, it feems next to certain that they were produced by fublimation.

The old Sable Siberian iron is very tough at high heats.

The very powerful effects produced by this laft barrel, the fize of which (reduced, indeed, by repeated oxidation) was not above an inch fquare, made me very anxious to obtain barrels of the fame fubftance, which being made of greater fize, ought to afford refults of extreme intereft. I found upon inquiry, that this barrel was not made of Swedifh iron, as I at firft fuppofed, but of what is known by the name of *Old Sable*, from the figure of a Sable ftamped upon the bars; that being the armorial badge of the place in Siberia where this iron is made.*

All iron is crufhed under the hammer at fome definite heat. Caft iron at a low heat;

A workman explained to me fome of the properties of different kinds of irons, moft interefting in my prefent purfuit; and he

* I was favoured with this account by the late Profeffor Robifon.

he illustrated what he said by actual trial. All iron, when exposed to a certain heat, crushes and crumbles under the hammer; but the temperature in which this happens, varies with every different species. Thus, as he shewed me, cast iron crushes in a dull red heat, or perhaps about 15° of Wedgwood; steel, in a heat perhaps of 30°; Swedish iron, in a bright white heat, perhaps of 50° or 60°; old sable itself, likewise yields, but in a much higher heat, perhaps of 100°. I merely guessed at these temperatures; but I am certain of this, that in a heat similar to that in which Swedish iron crumbled under the hammer, the old sable withstood a strong blow, and seemed to possess considerable firmness. It is from a knowledge of this quality, that the blacksmith, when he first takes his iron from the forge, and lays it on the anvil, begins by very gentle blows, till the temperature has sunk to the degree in which the iron can bear the hammer. I observed, as the strong heat of the forge acted on the Swedish iron, that it began to boil at the surface, clearly indicating the discharge of some gaseous matter; whereas, the old sable, in the same circumstances, acquired the shining surface of a liquid, and melted away without any effervescence. I procured, at this time, a considerable number of bars of that iron, which fully answered my expectations.

steel at a higher, Swedish iron at a bright white heat, and old Sable at a still higher temperature.

By the experiments last mentioned, a very important point was gained in this investigation; the complete fusibility of the carbonate under pressure being thereby established. But from this very circumstance, a necessity arose of adding some new devices to those already described: for the carbonate, in fusion, spreading itself on the inside of the tube containing it, and the two uniting firmly together, so as to be quite inseparable, it was impossible, after the experiment, to ascertain the weight of the carbonate by any method previously used. I therefore determined in future to adopt the following arrangement.

The complete fusibility of the carbonate under pressure was ascertained in these strong barrels.

A small tube of porcelain (*ik*, *Fig*. 23.) was weighed by means of a counterpoise of sand, or granulated tin; then the carbonate was firmly rammed into the tube, and the whole weighed again: thus the weight of the carbonate, previous to the experiment was ascertained. After the experiment, the tube, with its contents, was again weighed; and the variation of weight obtained, independently of any mutual action that had taken

Arrangement for obviating some difficulties which arose from the fusion. The carbonate was put into a small porcelain tube and this properly secured in a larger. These

were placed in a cradle or frame and the whole put into the iron barrel, &c.

taken place between the tube and the carbonate. The balance which I used, turned in a constant and steady manner, with one hundredth of a grain. When pounded chalk was rammed into this tube, I generally left part of it free, and in that space laid a small piece of lump-chalk (*i*), dressed to a cylinder, with the ends cut flat and smooth, and I usually cut a letter on each end, the more effectually to observe the effects produced by heat upon the chalk; the weight of this piece of chalk being always estimated along with that of the powder contained in the tube. In some experiments, I placed a cover of porcelain on the muzzle of the little tube, (this cover being weighed along with it), in order to provide against the case of ebullition: but as that did not often occur, I seldom took the trouble of this last precaution.

Continuation of the method of experiment.

It was now of consequence to protect the tube, thus prepared, from being touched during the experiment, by any substance, above all, by the carbonate of lime, which might adhere to it, and thus confound the appreciation by weight. This was provided for as follows: The small tube (*Fig.* 23, *i k*) with its pounded carbonate (*k*), and its cylinder of lump-chalk (*i*), was dropt into a large tube of porcelain (*p k*, *Fig.* 24). Upon this a fragment of porcelain (*l*), of such a size as not to fall in between the tubes, was laid. Then a cylinder of chalk (*m*) was dressed, so as nearly to fit and fill up the inside of the large tube, one end of it being rudely cut into the form of a cone. This mass being then introduced, with its cylindrical end downwards, was made to press upon the fragment of porcelain (*l*). I then dropped into the space (*n*), between the conical part of this mass and the tube, a set of fragments of chalk, of a size beyond what could possibly fall between the cylindrical part and the tube, and pressed them down with a blunt tool, by which the chalk being at the same time crushed and rammed into the angle, was forced into a mass of some solidity, which effectually prevented any thing from passing between the large mass of chalk and the tube. In practice, I have found this method always to answer, when done with care. I covered the chalk, thus rammed, with a stratum of pounded flint (*o*), and that again with pounded chalk (*p*) firmly rammed. In this manner, I filled the whole of the large tube with alternate layers of silex and chalk; the muzzle being always occupied with chalk, which was easily pressed into a mass of tolerable firmness

firmness, and, suffering no change in very low heats, excluded the fusible metal in the first stages of the experiment. Continuation of the method of experiment.

The large tube, thus filled, was placed in the cradle, sometimes with the muzzle upwards, and sometimes the reverse. I have frequently altered my views as to that part of the arrangement, each mode possessing peculiar advantages and disadvantages. With the muzzle upwards, (as shewn in *Fig.* 24 and 25), the best security is afforded against the intrusion of the fusible metal; because the air, quitting the air-tube in the working position, occupies the upper part of the barrel; and the fusible metal stands as a liquid (at *q*, *Fig.* 25.) below the muzzle of the tube, so that all communication is cut off, between the liquid metal and the inside of the tube. On the other hand, by this arrangement, the small tube, which is the fiducial part of the apparatus, is placed at a considerable distance from the breech of the barrel, so as either to undergo less heat than the upper part, or to render it necessary that the barrel be thrust high into the muffle.

With the muzzle of the large tube downwards, the inner tube is placed (as shewn in *Fig.* 22), so as still to have its muzzle upwards, and in contact with the breech of the large tube. This has the advantage of placing the small tube near to the breech of the barrel: and though there is here less security against the intrusion of liquid metal, I have found that a point of little consequence; since, when the experiment is a good one, and that the carbonic acid has been well confined, the intrusion seldom takes place in any position. In whichever of the two opposite positions the large tube was placed, a pyrometer was always introduced, so as to lie as near as possible to the small tube. Thus, in the first-mentioned position, the pyrometer was placed immediately below the large tube, and, in the other position above it; so that, in both cases, it was separated from the carbonate by the thickness only of the two tubes.

Much room was unavoidably occupied by this method, which necessarily obliged me to use small quantities of carbonate, the subject of experiment seldom weighing more than 10 or 12 grains, and in others far less*.

On

* I measured the capacity of the air-tubes by means of granulated tin, acting as a fine and equal sand. By comparing the weight of

Experiment made with the foregoing precautions. The heat amounted to 64° Wedgwood. The carbonates had lost gas and undergone fusion.

On the 11th of April, 1803, with a barrel of old fable iron having a bore of 0.75 of an inch, I made an experiment in which all thefe arrangements were put in practice. The large tube contained two fmall ones; one filled with fpar, and the other with chalk. I conceived that the heat had rifen to 33°, or fomewhat higher. On melting the metals, the cradle was thrown out with confiderable violence. The pyrometer, which, in this experiment, had been placed within the barrel, to my aftonifhment, indicated 64°. Yet all was found. The two little tubes came out quite clean and uncontaminated. The fpar had loft 17.0 *per cent.* the chalk 10.7 *per cent.* The fpar was half funk down, and run againft the fide of the little tube: Its furface was fhining, its texture fpongy, and it was compofed of a tranfparent and jelly-like fubftance. The chalk was entirely in a ftate of froth. This experiment extends our power of action, by fhewing, that compreffion, to a confiderable degree, can be carried on in fo great a heat as 64°. It feems likewife to prove, that, in fome of the late experiments with the fquare barrel, the heat had been much higher than was fuppofed at the time, from the indication of the pyrometer placed on the breech of the barrel; and that in fome of them, particularly in the laft, it muft have rifen at leaft as high as in the prefent experiment.

Experiment in which the barrel failed after its contents had undergone fufion.

On the 21ft of April, 1805, a fimilar experiment was made with a new barrel, bored in a fquare bar of old fable, of about two and a half inch in diameter, having its angles merely rounded; the inner tube being filled with chalk. The heat was maintained during feveral hours, and the furnace allowed to burn out during the night. The barrel had the appearance of foundnefs, but the metals came off quietly, and the carbonate was entirely calcined, the pyrometer indicating 63°. On examination, and after beating off the fmooth and even fcale of oxide peculiar to the old fable, the barrel was found to have yielded in its peculiar manner; that is, by the opening of the longitudinal fibres. This experiment, notwithftanding the failure of the barrel, was one of the moft interefting I had

of this tin with an equal bulk of water, I found that a cubic inch of it weighed 1330.6 grains, and that each grain of it correfponded to 0.00075 of a cubic inch. From thefe data I was able, with tolerable accuracy, to gage a tube by weighing the tin required to fill it.

made;

made, since it afforded proof of complete fusion. The carbonate had boiled over the lips of the little tube, standing, as just described, with its mouth upwards, and had run down to within half an inch of its lower end: most of the substance was in a frothy state, with large round cavities, and a shining surface; in other parts, it was interspersed with angular masses, which have evidently been surrounded by a liquid in which they floated. It was harder, I thought, than marble; giving no effervescence, and not turning red like quicklime in nitric acid, which seemed to have no effect upon it in the lump. It was probably a compound of quicklime with the substance of the tube.

With the same barrel repaired, and with others like it, many similar experiments were made at this time with great success; but to mention them in detail, would amount nearly to a repetition of what has been said. I shall take notice of only four of them, which, when compared together, throw much light on the theory of these operations, and likewise seem to establish a very important principle in geology. These four experiments differ from each other only in the heat employed, and in the quantity of air introduced.

Account of some experiments at very elevated heats. The carbonate most heated was calcined: that which had suffered less heat had the form of lime-stone and of marble, which retained their carbonic acid.

The first of these experiments was made on the 27th of April 1803, in one of the large barrels of old sable, with all the above-mentioned arrangements. The heat had risen, contrary to my intention, to 78° and 79°. The tubes came out uncontaminated with fusible metal, and every thing bore the appearance of soundness. The contents of the little tube, consisting of pounded chalk, and of a small piece of lump-chalk, came out clean, and quite loose, not having adhered to the inside of the tube in the smallest degree. There was a loss of 41 *per cent.* and the calcination seemed to be complete; the substance, when thrown into nitric acid, turning red, without effervescence at first, though, after lying a few minutes, some bubbles appeared. According to the method followed in all these experiments, and lately described at length, (and shewn in *Fig.* 24 and 25), the large tube was filled over the small one, with various masses of chalk, some in lump, and some rammed into it in powder; and in the cradle there lay some pieces of chalk, filling up the space, so that in the cradle there was a continued chain of carbonate of four or five inches in length. The substance was found to be less and less calcined, the more it

it was removed from the breech of the barrel, where the heat was greateſt. A ſmall piece of chalk, placed at the diſtance of half an inch from the ſmall tube, had ſome ſaline ſubſtance in the heart, ſurrounded and intermixed with quicklime, diſtinguiſhed by its dull white. In nitric acid, this ſubſtance became red, but efferveſced pretty briſkly; the efferveſcence continuing till the whole was diſſolved. The next portion of chalk was in a firm ſtate of limeſtone; and a lump of chalk in the cradle, was equal in perfection to any marble I have obtained by compreſſion: the two laſt-mentioned pieces of chalk efferveſcing with violence in the acid, and ſhewing no redneſs when thrown into it. Theſe facts clearly prove, that the calcination of the contents of the ſmall tube had been internal, owing to the violent heat which had ſeparated its acid from the moſt heated part of the carbonate, according to the theory already ſtated. The ſoundneſs of the barrel was proved by the complete ſtate of thoſe carbonates which lay in leſs heated parts. The air-tube in this experiment had a capacity of 0.29, nearly one-third of a cubic inch.

Another experiment in which the barrel failed.

The ſecond of theſe experiments was made on the 29th of April, in the ſame barrel with the laſt, after it had afforded ſome good reſults. The air-tube was reduced to one-third of its former bulk, that is, to one-tenth of a cubic inch.—The heat roſe to 60°. The barrel was covered externally with a black ſpongy ſubſtance, the conſtant indication of failure, and a ſmall drop of white metal made its appearance. The cradle was removed without any exploſion or hiſſing. The carbonates were entirely calcined. The barrel had yielded, but had reſiſted well at firſt; for the contents of the little tube were found in a complete ſtate of froth, and running with the porcelain.

Third experiment, very thin fuſion.

The third experiment was made on the 30th of April, in another ſimilar barrel. Every circumſtance was the ſame as in the two laſt experiments, only that the air-tube was now reduced to half its laſt bulk, that is, to one-twentieth of a cubic inch. A pyrometer was placed at each end of the large tube. The uppermoſt gave 41°, the other only 15°. The contents of the inner tube had loſt 16 *per cent.* and were reduced to a moſt beautiful ſtate of froth, not very much injured by the internal calcination and indicating a thinner ſtate of fuſion than had appeared.

The

The fourth experiment was made on the 2d of May, like the rest in all respects, with a still smaller air-tube, of 0.0318, being less than one thirtieth of a cubic inch. The upper pyrometer gave 25°, and the under one 16°: The lowest masses of carbonate were scarcely affected by the heat. The contents of the little tube lost 2.9 *per cent.* both the lump and the pounded chalk were in a fine saline state, and, in several places, had run and spread upon the inside of the tube, which I had not expected to see in such a low heat. On the upper surface of the chalk rammed into the little tube, which, after its introduction had been wiped smooth, were a set of white crystals, with shining facettes, large enough to be distinguished by the naked eye, and seeming to rise out of the mass of carbonate. I likewise observed, that the solid mass on which these crystals stood, was uncommonly transparent.

Fourth experiment under greater pressure, fusion at a moderate heat, slight loss of carbonic acid, &c.

In these four experiments, the bulk of the included air was successively diminished, and by that means its elasticity increased. The consequence was, that in the first experiment, where that elasticity was the least, the carbonic acid was allowed to separate from the lime, in an early stage of the rising heat, lower than the fusing point of the carbonate, and complete internal calcination was effected. In the second experiment, the elastic force being much greater, calcination was prevented, till the heat rose so high as to occasion the entire fusion of the carbonate, and its action on the tube, before the carbonic acid was set at liberty by the failure of the barrel. In the third experiment, with still greater elastic force, the carbonate was partly calcined, and its fusion accomplished, in a heat between 41° and 15°. In the last experiment, where the force was strongest of all, the carbonate was almost completely protected from decomposition by heat, in consequence of which it crystallized and acted on the tube, in a temperature between 25° and 16°. On the other hand, the efficacy of the carbonic acid as a flux on the lime, and in enabling the carbonate to act as a flux on other bodies, was clearly evinced; since the first experiment proved that quicklime by itself could neither be melted, nor act upon porcelain, even in the violent heat of 79°; whereas, in the last experiment where the carbonic acid was retained, both of these effects took place in a very low temperature.

Observations: the fusion takes place at lower heats when the escape of carbonic acid is prevented. The acid acts as a flux.

(To be continued.)

IV. Observations

IV.

Obſervations on the Effect of Madder Root on the Bones of Animals. By Mr. B. GIBSON.*

Account of the firſt diſcovery of the property of madder to tinge the bones of living animals.

THERE is, perhaps, no phenomenon, which occurs in an animal body more curious, than the tinge communicated to the bones of living animals, whoſe food has been mixed with madder root. This, like many other facts, to which no reaſoning *à priori* could have directed us, was diſcovered by chance. Mr. Belcher, dining with a calico printer on a leg of freſh pork, was ſurprized that the bones, inſtead of poſſeſſing their uſual whiteneſs, were of a deep red colour; and on enquiring the cauſe of it, was informed, that the pig had been fed upon the refuſe of the dyers' vats, and had received ſo much of the colouring matter of madder into the ſyſtem, that its bones were dyed by it. So intereſting a fact has attracted very much the attention of anatomiſts, and has been uſed in many phyſiological and pathological enquiries; it may not therefore be unintereſting to give a ſhort hiſtory of the phenomena connected with it, and the purpoſes to which it has been applied, previous to entering upon the more immediate object of this paper.

Experiments ſhewing that the tinge is more quickly given to the bones of growing animals.

Many experiments have been made to aſcertain how long a time is required to produce the tinge, and whether it be permanent or only temporary. Belcher and Morand, about the ſame time, mixed madder root with the food of chickens and young pigeons. The reſult of their obſervations was, that the tinge was more quickly communicated to the bones of growing animals, than to the bones of animals which had already completed their growth; the bones of young pigeons being tinged of a roſe-colour in twenty-four hours, and of a deep ſcarlet in three days; whilſt the bones of adult animals only exhibited a roſe-colour in fifteen days. They found the tinge moſt intenſe in the ſolid parts of thoſe bones, which were neareſt to the centre of circulation; whilſt in bones of equal ſolidity, at a greater diſtance from the heart, the tint was more faint. The dye was deep in proportion to the length of time the madder had been continued, and when it was diſcontinued, the colour gradually became more and more faint, till

Short time and other facts.

* Mancheſter Memoirs, 1805.

it entirely disappeared. According to the experiments of these gentlemen, other vegetable dyes, such as logwood, turmeric and alkanet root, did not communicate their respective tints to the bones.*

Du Hamel used this property to shew the growth of bones.

This effect of madder upon the bones, was soon afterwards made use of by Du Hamel, in his attempt to prove the manner in which the bones of animals are encreased in thickness. Observing in the vegetable kingdom, that the bark, by a sort of secretion, formed the ligneous part of a tree, in successive layers; so he conceived that the periosteum, or membrane surrounding bones, being converted into osseous matter, encreased their diameter by adding to them concentric laminæ in succession. In order to prove the justness of his opinion, he mixed the food of a cock with madder root for a month, withheld it for a month, and then gave it again. He afterwards killed the animal, and upon inspection thought he observed the appearance which he expected; viz. two layers of red bone inclosing one of white, corresponding to the periods of the madder's being given or withheld.

It is very doubtful whether that growth could be so indicated.

This experiment, and some others related by Du Hamel, appear to be conclusive in favour of the theory, which he wished to establish; and as they were conducted by a physiologist of high character, the accuracy of the observations could not have been doubted, had these experiments stood alone. But when they are compared with some of his own previous experiments, and those of other authors, it is difficult to reconcile them. In some of Du Hamel's experiments, for instance, the bones of a cock were tinged of a rose-colour through their whole substance in sixteen days, and those of young pigeons of a deep scarlet in three days. In several ex-

* From some experiments I made on young pigeons, I found that a considerable quantity of logwood, in the form of extract, communicated an evidently purple tint to the bones. With regard to turmeric, it appears to be altered in its colour by passing through the digestive organs, for the fæces of the animals, who took it in considerable quantity, were constantly green: whilst either logwood or madder root exhibited their respective hues after passing through the intestines. Saffron exhibits properties different from any of these substances; for though a pigeon took it in considerable quantity, and thereby had its fæces tinged, yet no perceptible alteration of colour was produced in its bones.

 periments

periments I have made on the subject, I have found the bones of young pigeons tinged of a uniform rose-colour, internally as well as externally, in twenty-four hours. This communication of colour to the whole substance of the osseous system in so short a time, makes it highly improbable that the laminated appearance, remarked by Du Hamel, was produced by the new formation of red and white osseous layers, corresponding to the times (months) the madder had been given or withheld. For, as Mr. John Bell very justly remarks,* "If a bone should increase by layers thick enough to be visible and of a distinct tint, and such layers be continually accumulated upon each other every week, what kind of bone should this grow to?" The only way in which we can reconcile with each other the phenomena observed in the different experiments, and account for their apparent contradiction, is, by supposing that Du Hamel mistook for an obscurely laminated appearance, the variety in the tint, which is more deeply communicated to the more solid, and more faintly to the less compact parts of a bone.

Late experiments of Dr. M'Donald on the bones.

This property of madder of tinging the bones of animals, has lately been employed by Dr. M'Donald,† in his ingenious researches into the formation and death of bones. Amongst other objects, he attempted to ascertain in what manner and how soon a cylindrical bone is regenerated to supply the place of one artificially killed. As the process is highly curious, I shall briefly relate the principal points.

Very curious process of a bone destroyed,

Dr. M'Donald's experiments were made by amputating the proper leg-bone of young pigeons or chickens immediately above the joint. The marrow was then extracted, and the cavity which contained it, filled with lint. This process caused the death of the bone, and the formation of a new bone surrounding that destroyed ensued. Immediately after the experiment, the animal had its food mixed with madder root, and the part was inspected in different animals, at different periods.

—and the regular process of

On examination three days afterwards, the periosteum or enveloping membrane, was found much thickened; and underneath it a gelatinous humour was effused, surrounding the

* Anatomy of the bones, &c. p. 15.

† Disputatio inauguralis de Necrosi ac Callo. 1799.

dead

dead bone, and ſpotted with red oſſeous nuclei; proving that the regeneration of the bone had commenced at this early period.

In ſeven days the new bone was found ſoft and flexible, not to be diſtinguiſhed from cartilage or griſtle, except by the red tint the madder had communicated to it; yet the bone deſtroyed was not at all coloured, although the other bones of the animal had acquired a bright red. From this time the new bone continued to encreaſe in hardneſs, ſurrounding the old one like a ſheath. The latter in about three weeks was ſo looſe as to be drawn out, and in about fifteen days from this time, the cavity of the regenerated bone was filled with marrow, and in every reſpect performed the office of that for which it was a ſubſtitute. This may be conſidered as a general outline of the progreſſive changes which take place during the regeneration of a cylindrical bone, in a young animal, ſuch as a pigeon, or chicken; and the ſame proceſs is frequently performed in the human body, when, from ſome internal cauſe, the life of a bone is deſtroyed. Theſe changes involve many intereſting particulars; but the circumſtance moſt immediately connected with the ſubject of this paper is, that although the ſhaft of the bone required three weeks for its renewal, yet in ſeven days the oſſeous ſyſtem generally had acquired a bright red. Now if we explain this change in colour according to the common opinion of abſorption of the white, and depoſition of the red oſſeous matter,* we muſt neceſſarily draw this concluſion; that the oſſeous ſyſtem of the animal will be renewed three times during the period, which the formation of the ſubſtitute bone requires; a concluſion which we ſhould be inclined to reject merely from its improbability. But beſides this, the appear-

—its regeneration.

Inference. From the very ſpeedy acquiſition and ſubſequent loſs of the red colour that the oſſeous ſyſtem was naturally abſorbed and renewed in that period.

* The common opinion of phyſiologiſts, with regard to this curious fact, is, that when a bone becomes red, during the exhibition of madder root, the white oſſeous particles which compoſed it, have been entirely removed by abſorption and replaced by new oſſeous matter of a red colour: and when a bone aſſumes its natural colour, theſe red particles have been removed and replaced by white. If this be the fact, it neceſſarily follows, that an animal has at leaſt fifty-two new ſets of bones in a year: for the oſſeous ſyſtem, according to the experiments of the moſt reſpectable phyſiologiſts, acquires a deep red tint from madder in one week, and aſſumes its natural colour in another.

ance

ance of the parts strongly militate against it—for, if we may judge at all of the activity of the process in the two parts, by their comparative degrees of vascularity, that employed in forming the substitute bone far exceeds that going on in the osseous system generally; one striking phenomenon attending the regeneration of a bone being, the very high degree of increased vascularity which the parts employed in the process rapidly assume.

Cause of doubt.

The bones are alone reddened by madder, because the phosphate of lime acts as a mordent on the madder.

After this effect of madder upon the bones was known, it long remained a mystery, why some other white parts of the body, such as nerves, cartilages and periosteum, were not equally liable to be coloured by it, as the bones. This fact, I believe, did not receive any explanation, until Dr. Rutherford gave a very ingenious and satisfactory one. When speaking of this property of madder, he says,* "We have, in the fact before us, a beautiful example of a particular case of chemical attraction; such as in numberless instances, is observed to take place between the colouring particles of both animal and vegetable substances and various other bodies, especially earths and earthy salts, and oxides of metals. So strong is the affinity of the colouring matter to these bodies, that it is frequently observed to quit the menstruum, in which it may chance to be dissolved, to unite with them: they, in consequence of its union, acquiring a particular tinge, whilst the menstruum is proportionably deprived of colour. From this principle, this mutual attraction, is deduced the various use of those bodies as mordents, as they are called, intermedia, or means for fixing the colours in dying or staining thread or cloth, whether it be composed of animal or vegetable materials. Upon the same principle depends the preparation of those pigments known to painters under the name of lakes; these are truly precipitates of the colouring matter, in combination with various mordents, as their basis.—The colouring of the bones of a living animal by means of madder, is, in every circumstance, analogous to the formation of these lakes. The colouring matter of madder, passing unaltered through the digestive organs of the animal, enters the general mass of fluids, and is dissolved in the serum of the blood, to which,

The red matter is a kind of lake.

* See Dr. Blake's inaugural Dissertation. De dentium formatione, p. 11?.—1798.

indeed,

indeed, if it be in large proportion, it communicates a sensibly red tinge. *But there is always present in the blood, and in state of solution in the serum, a quantity of the earthy matter of the bones, phosphate of lime, ready to be deposited, as the exigencies of the animal may require. Now the phosphate of lime is an excellent mordent to madder, and has a strong affinity to it, and is consequently admirably fitted to afford a base for the colouring matter of it: in such experiments, therefore, they concrete in the state of a bright red lake, whence the colour of the bones is derived.* That this is actually the case, may be shewn by a variety of experiments. Thus, if to an infusion of madder in distilled water, be added a little of the muriate of lime, no change is perceived: but if to this mixture be added a solution of the phosphate of soda, immediately a double elective attraction takes place. The muriatic acid combining with the soda, remains suspended, or dissolved in the water; whilst the phosphoric acid, thus deprived of its soda, combines with the lime which the muriatic acid parted with, and forms phosphate of lime or earth of bones. This substance however, being insoluble in water, falls to the bottom; but having combined at the instant of its formation with the colouring matter of the madder, they fall down united into a crimson lake; precisely of the same tint with that of the bones of young animals, which have been fed with madder. From this simple representation of the matter, we have a ready explication of every circumstance which has been remarked as extraordinary respecting this subject."

—formed as it seems before the osseous deposition.

Whilst Dr. Rutherford thus gives a most satisfactory explanation of the colour of madder being communicated to the bones alone, of all the white parts of an animal; we find that he embraces the same opinion as other physiologists, that the osseous materials acquire their colour previous to their deposition, whilst in a state of solution or mixture in the blood; from whence they are afterwards deposited, and concrete in the form of a bright lake. In no part of his ingenious remarks does he hint at the probability that the bones already formed in an animal, may, during the use of madder, become red, and after its disuse gradually resume their natural colour, by the agency of a power entirely independent of their deposition and absorption: that this is probable I shall now proceed to prove.

Dr. Rutherford admits the absorption and deposition.

Before

More particular explanation of the doctrine of the absorption and regeneration of the parts of animals,

Before it was discovered that madder possessed this property of tinging bones, physiologists had long been of opinion, that the various parts of the body, being worn out by the performance of their actions and functions, were gradually removed, and replaced by new materials. They had seen, as Mr. J. Bell observes, the whole osseous system by the morbid removal of its solid part, rendered so soft and flexible as to bend under the common weight of the body and ordinary action of parts; the regeneration of many bones which had been destroyed by disease; the rapid absorption of fat in some diseases, and its speedy reproduction; and lastly, the gradual change which the fluids of the body undergo, as well as some of its insensible parts, the hair and nails; hence they supposed that the same process of change and renovation went on in every organ, and that the bodies of animals were not composed of the same identical particles of which they would consist at some future period. This process, which was before but conjectural, or supported by analogy, physiologists considered as fully proved by the effects of madder upon the bones. They had by this means an opportunity of seeing the bones altered in colour, from the slightest tint to the deepest red; they could observe this gradually removed, until the bones had regained their natural whiteness; and explaining the whole process on the principle of deposition and absorption, they considered it as ocular demonstration of a most rapid change in the constituent elements of a part, of which, from its solidity, they could scarcely have believed it susceptible.

—supposed to be confirmed in the bones.

Probability that this explanation is erroneous.

I apprehend, however, that it is by giving an erroneous explanation of the phenomena; by supposing that a change in the osseous particles is denoted by an alteration in their colour, that physiologists have considered this fact as conclusive.—— However indubitable and well supported may be the opinion, which attributes an imperceptible change to the various parts of the body, we shall, I believe discover upon a more close examination, that it is by no means supported by the appearances, which the bones display on the exhibition of madder root. The rapid change in their particles, which such appearances indicate, when explained in the common way, is completely at variance with all the processes performed by the bones, both in their healthy and diseased states. Thus we find the formation of the ossific matter, called Callus, for the

union

union of fractured bones, or the exfoliation of a part of a bone, are processes requiring a considerable length of time for their performance. In Dr. M'Donald's experiments, the formation of a regenerated bone required nearly six weeks; but during the same space of time, the bones of the same animal would be renewed several times, if the common explanation of the communication and disappearance of the tinge of madder were well founded. From these circumstances, I am led to believe that the appearances produced by the exhibition of madder, require another mode of explanation. That which I have to offer is not liable to the same objections, and is strongly supported by comparative experiments.

For the processes by which bones are restored are slow.

It was observed by Du Hamel, in his experiments, that the bones of animals which had been deeply tinged by madder, by long exposure to air lost their colour and became white.—It was this fact which suggested to me a simple explanation of the process. It occurred to me, that if any one of the component parts of the blood naturally exerted a stronger attraction for the colouring matter of madder, than the phosphate of lime, it might be deprived of the tint by a chemical power. In order to prove this, as far as I could by experiment, I took one dram of the phosphate of lime tinged, as in Dr. Rutherford's experiment, and exposed it for half an hour to the action of two ounces of fresh serum, at the temperature of 98 degrees. By this operation, the serum gradually acquired a red tinge, whilst the phosphate of lime was proportionably deprived of colour. In a comparative experiment, a similar quantity of tinged phosphate of lime was exposed to the action of distilled water under similar circumstances; but no change took place. The knowledge of this strong affinity in the serum for colouring matter, affords an easy and simple explanation of the effects of madder on the bones, upon the principle of chemical attraction.

A simple explanation grounded on experiment.

The serum of blood has a stronger attraction for the colouring matter of madder than phosphate of lime has.

Thus, when an animal has madder mixed with its food, the blood becomes highly charged with it, and imparts the superabundant colouring matter to the phosphate of lime, contained in the bones already formed; as it circulates through them and moistens them throughout. But as soon as an animal has ceased to receive the madder, and the blood is freed from the colouring matter by the excretions, the serum then exerts its superior attraction, and by degrees entirely abstracts it from the

Hence the bones are dyed when much madder is in the system, and bleached by the serum when the quantity becomes less.

the phosphate of lime, and the bones resume their natural whiteness. In short, the bones are at one time dyed by the colouring matter, at another time bleached by the serum.

Phosphate while suspended does not strongly take the colouring matter.

Whilst I have attempted to explain the probable manner in which the bones, *already formed* in an animal, at one time receive, and at another are deprived of the colouring matter of madder, I by no means intend to assert that the phosphate of lime does not acquire a similar colour during its solution in the serum, or at the time it is precipitated from it to enter into the composition of the bones; the fact is indisputable. I have, however, found from some experiments lately made upon a hen during oviparation, that only a slight tinge can be communicated to the shell, formed whilst a large quantity of colouring matter is circulating with the blood. So slight indeed is the blush, that it would not be seen by a common observer, unless contrasted with a natural egg: which is probably the reason why it has, I believe, been denied by physiologists, that the shell of an egg is altered by the exhibition of madder. If this may be considered as a test of the quantity of colouring matter, which the phosphate attracts at the time it is separated from the blood, it forms another strong argument against the theory, which Dr. Rutherford, and all preceding physiologists have adopted; for, consistent with this fact, the bones should never exhibit more than a slight blush. When explained upon the principle of chemical attraction, we see that the phenomena, exhibited by the bones of an animal, by giving or withholding madder root, give no support to the opinion that the various parts of the body continually undergo an imperceptible change; and I consider it a fortunate circumstance for that doctrine that so simple an explanation of the effect of madder can be given. For whilst so specious a fact has been considered, by the highest authorities, as complete proof of the imperceptible renovation of parts; the rapid change in the constituent elements of the bones, which the communication and disappearance of the colour indicates, must have appeared astonishing to every physiologist. Of this I cannot give you a stronger instance than in the words of Mr. J. Bell.* "Nothing," says he, "can be more curious than this continual renovation and change of parts even in the hardest bones. We are accustomed to say of the whole body, that it is daily

Example in eggs.

The doctrine of a rapid and continual change is not supported by the facts of bones tinged by the madder.

* Anatomy of the bones, &c. p. 13.

changed.

cl ; that the older particles are removed, and new ones their place; that the body is not now the same individual body, that it was; but it could not be easily believed that we speak only by guess concerning the softer parts, which we know for certain of the bones.—When madder is given to animals, withheld for some time and then given again, the colour appears in their bones, is removed, and appears again with such a sudden change, as proves a rapidity of deposition and absorption exceeding all likelihood or belief; all the bones are tinged in twenty-four hours; in two or three days their colour is very deep, and if the madder be left off but for a few days, the red colour is entirely removed."

Although by this chemical explanation of the effect of madder upon the bones, the doctrine of the imperceptible change in the component parts of animal bodies, loses the support of a fact, which has, since its discovery, been universally considered as its strongest proof; nevertheless, indisputable arguments, derived from different sources, still place that doctrine amongst the best supported opinions in physiology.

V.

On Fairy Rings and the Waste of Fish in Scotland. By A. T.

To Mr. NICHOLSON.

SIR,

HAVING frequently noticed the fairy-rings your correspondent, M. Florian Jolly mentions in your Journal for February, I should be glad to know from him whether hares or rabbits abounded in Broadlands park, as I have generally observed these rings most prevalent, in light sandy soils, particularly among rabbit burrows. This species of soil from its dryness would be very unfavourable to the idea of these things being formed from a central heap of horse dung; besides, were this the cause of them, we should expect them to be always circular, or when not circular, that those parts most remote from the centre would appear not to have benefited so strongly from the manure as those which were nearer. I have generally observed that the rings were composed of a double circle, or rather a little circular path, the middle of which appeared to be

Observations and inquiry whether fairy ring may not have been made by hares and rabbits.

be trodden, and the edges grown up, and more in igour than any of the ſurrounding graſs. I had occaoſin to remark one of thoſe fairy-rings laſt ſummer: it was perfectly circular, and about ten feet in diameter, it was ſituated at the edge of a copſe wood, and in a vicinity where there are abundance of both hares and rabbits; but what appeared to me moſt ſingular, was its being interſected exactly through the middle, by a well frequented foot path. The hare is rather given to gravity, the rabbit is more playful; but whether it is given to the amuſement of *lounging in the ring*, ſome of your more informed correſpondents may be enabled to inform you.

Fiſh is undoubtedly waſted in Scotland.

I OBSERVE ſome of your correſpondents have got into a controverſy reſpecting the waſte of fiſh in Scotland. No doubt can exiſt upon that head; not however ariſing from the waſteful diſpoſition of the natives, their delicacy in appetite or ſuperabundance of proviſion, but from the want of a market for the conſumption of their overplus. To talk of Aberdeen fiſhermen bringing freſh fiſh to Newcaſtle, Norwich, or Leeds, is as ridiculous as to propoſe taking them to Amſterdam, or London; for beſides the difficulty of again making their own ports, they will conſtantly find an over-ſtocked market, as the ſame weather that permits them to fiſh will permit their neighbours to do the ſame. But the grand cauſe of all the waſte is the horrible monopoly which their country labours under in reſpect to their ſalt laws, where for the ſake of a few paltry pans, Engliſh ſalt is excluded under the ſevereſt penalties, although it can be delivered in any part of Scotland at one half the price that we are forced to pay for Scotch ſalt under the preſent circumſtances. Give them ſalt at a cheap rate, if it does not permit them to export the fiſh, as that requires capital and new eſtabliſhments, it would at leaſt enable them to ſupply the interior; a thing as worthy the attention of the public as the ſupply of any other market I know.

The fiſheries of Scotland are deſtroyed by the operation of the ſalt laws.

Your moſt obedient

A. T.

March 23, 1803.

VI.

Letter from AMICUS *respecting the supposed Waste of Crab-Fish in Scotland.*

To Mr. NICHOLSON.

SIR,

THE very respectable and distinguished rank which the Philosophical Journal holds among the periodical publications will at all times prevent its becoming the vehicle of unnecessary dispute or contradiction: yet as public information and utility is sometimes promoted by the correction of mistakes, when this is likely to be the case, any thing that can elucidate a fact either misrepresented or partially stated, is doubtless compatible with the spirit of your publication. In your 48th number it is stated by "an Enquirer" that the crab fishery is so productive about Arbroath that, after boiling them, the bodies of the crabs are thrown away, and the large claws only brought to table, of which the Enquirer says he has been a witness. The fact is literally true, but wants further explanation. It is well known to every person resident on the coasts where crab-fish are commonly to be had, that many of that species are scarcely eatable, being often found after boiling to contain hardly any thing but water. The writer of this article has repeatedly seen from twelve to twenty crabs boiled at one time, and every one of them, more or less, in the above situation. When this is the case, the meat of the great claws (although they still may be eaten) is also watery and insipid compared to those of a good crab, the body of which is filled with a very rich substance, which is so far from being thrown away, that it is in general esteemed a luxury, even where crabs are plenty. Some persons are, indeed, fond of the claws, who cannot eat the bodies at all; but these are only exceptions from general taste and common practice. The claws of a good crab (as has been already observed) are much firmer, more rich, and sweet to the taste than those of an inferior kind, which are by far the most abundant. The claws of the male are larger in proportion than those of the female: the male crab is also reckoned superior in quality, except for a very short period (in what time of the year I have not been able to

Observations concerning the fact that the bodies of crab-fish are wasted at Arbroath. It is a bad species which is rejected, the good ones are eaten.

to ascertain) when in the the opinion of some who pretend to be connoisseurs, the females are equal, or nearly so in this case.

Crabs are in season nine months in the year; May, June, and July are the only months in which they are not. Some piscatory epicures pretend to certain marks for distinguishing good crabs, but they are very far from being infallible; perhaps the most general distinction is, that a good crab has a shell of a dusky red colour, with a certain degree of roughness, particularly on the claws; while the bad ones have shells white, clear, smooth, and watery; but the distinction is much better understood from observation than any detailed account. Trusting that you, Mr. Editor, will have the goodness to insert this communication, and that your correspondent, "the Enquirer," will do me the justice to believe that my sole motive for troubling you was to give information, I am with esteem, SIR,

Your most obedient servant,

AMICUS.

Arbroath, March 1, 1806.

VII.

Probability that the Hindoos were acquainted with Saturn's Ring.

To Mr. NICHOLSON.

SIR,

I TAKE the liberty of requesting the insertion of the following quotation in your Philosophical Journal, from the 6th vol. of Mr. Maurice's Indian Antiquities, page 605. If it does really mean the ring of the planet Saturn, perhaps some of your readers can explain how it could have been discovered by the Brahmins in such remote ages.

Your's respectfully,

April 7, 1806. A. B. C.

Extract from Maurice's Indian Antiquities.

"I have already intimated in a former volume, that the *circle* formed around SANI (the Saturn of the Hindoos) by intertwining serpents, was probably intended to denote his RING. I have since had the figure engraved for the reader's inspection and decision. It is impossible to ascertain the exact age of the pictured image in the Pagoda, from which the portrait was taken;

taken; but probably both are of a very remote age, for the Indian pagodas are not fabrications of yesterday, nor in their conceptions and designs are they given to frequent vicissitude. If Sani were thus designated in very antient periods, the fact proves that they must, by what means can scarcely be conjectured, have discovered the phenomenon of his *ring*, for what besides could that serpentile oval inclosing the body of Sani be intended to represent? That phenomenon however was not known in Europe till about the year 1628, when Galileo, with the first perfect telescope discovered, what he conceived to be two stars at the extreme parts of the planet, but which in reality proved to be the *ansæ* of that ring, the natural existence of which was afterwards demonstrated by Heygens and succeeding astronomers. The circumstance is not the least wonderful of those that occur in the discussion of Indian antiquities and literature. I have stated the fact, and engraved the image; I leave to abler judges the task of decision."

VIII.

Explanation of Time keepers constructed by Mr. Thomas Earnshaw: for which a Reward of Three Thousand Pounds was awarded by the Commissioners of Longitude. From the Communications made by him to the Commissioners.*

THE model, from which the annexed drawings were taken contains, besides the parts necessary to explain the nature of the Escapement, a box inclosing a spring, which when wound up communicates, by means of some more wheels, a force to the balance-wheel, sufficient, when the balance is put in motion, to keep it in action for some time. These wheels are contained between two brass plates, fastened together by four upright

Description of the Escapement of Mr. Earnshaw's time piece.

* The Escapement with a model was communicated in June, 1804, and a subsequent explanation in March, 1805. The former is here given, and so much of the latter as directly relates to the time-keepers. The latter paper is no otherwise abridged than by omitting certain observations upon other artists, and some general remarks which do not form part of the disclosure.

I have been solicitous to give as early an account as might be proper, of the Escapements of Mr. Earnshaw and Mr. Arnold which

Defcription of the Efcapement of Mr. Earnfhaw's time piece.

upright pillars; the uppermoft of thefe plates is that which is reprefented by Fig. 1ft. plate XIII, where PQRS are the four fcrews that take into the heads of the four pillars above mentioned, and connect it to the remaining part of the model. The plate PQRS contains, however, the whole of the parts neceffary for the prefent purpofe. The fide of this plate reprefented to view, is the undermoft when fixed in the model; fo that the figure reprefents this plate as taken off, with the fide next to the balance laid upon a table, and the eye is fuppofed to be placed perpendicular over it.

In the plate PQRS is an opening, or a piece taken out, reprefented by TUWXYZ. In this opening, the balance-wheel ABCD, pallet MSK, and part of the balance UV are feen. The balance-wheel is fupported by two pieces of brafs, ONH, OI; the piece ONH is fcrewed to the fide of the plate neareft to view by a ftrong fcrew *t*, and made firm by fmall pins reprefented by $\pi\ \pi\ \pi\ \pi\ \pi\ \pi$; thefe pins are called fteady pins; they are riveted faft into the fupporting piece OH, and take into holes in the plate PQRS, made exactly to fit them. The part ON of this fupporting piece is fuppofed to be raifed above the part *t* H by a joint or bend at N; the other fupporting piece OI is faftened to the oppofite fide of the plate; and between thefe two pieces the balance-wheel turns freely and fteadily in the direction of the letters ABCD. The fmall wheel MSK is called the large pallet; it is a cylindrical piece of fteel, having a notch or piece cut out of it at *l h r*; againft the fide of this notch is a fquare flat piece of ruby, or any hard ftone, *h l*, ground and polifhed very fmooth, and fixed faft into the pallet. The cylinder is fo placed, with refpect to the balance-wheel, that it may not be more than juft clear of two adjoining teeth. EF is a long thin fpring, which

(which laft appears in No. 55 of our Journal) as they have been fo highly diftinguifhed by the national munificence. Some difcuffion of the important fubject of time pieces may be feen in the Philof. Journal, quarto feries, Vol. I. 56, and Vol. II. 106. As I expect fhortly to be favoured with a valuable communication refpecting the original inventors of free 'Scapements and compenfations, and may, according to circumftances, offer a few remarks on the fubject myfelf, I have been careful in the firft place to give the accounts of the above mentioned artifts in their own words.

W. N.

which is made faſt at one end, by being pinned into a ſtud, G, and made to bear gently againſt the head of an adjuſting ſcrew *m*; the other end is bent a little into the form of a hook; to this ſpring there is fixed another very ſlender ſpring at *γ*, which projects to a ſmall diſtance beyond it. This ſmall ſpring lies on the ſide of the thick ſpring neareſt to the balance-wheel. The adjuſting ſcrew, *m*, takes into a ſmall braſs-cock, *α p*, which is ſcrewed faſt to the plate PQRS by a ſtrong ſcrew at *p*. Upon the ſpring EF there is fixed a ſemi-cylindrical pin, which ſtands up perpendicular upon it, and of a ſufficient length to fall between the teeth of the balance-wheel ABCD. This pin is called the locking-pallet, and is placed on the oppoſite ſide of the ſpring repreſented to view. Through the center of the cylindrical pallet MSK, a ſtrong ſteel axis paſſes called the verge; the pallet is made faſt to this axis, which alſo paſſes through the center of the balance, and is made faſt to it; it has two fine pivots at its extremities, upon which it turns very freely, between two firm ſupporting pieces of braſs ſcrewed firmly, and made as permanent as poſſible, by ſteady pins to the principal plate PQRS; one of theſe pieces is repreſented in the figure by *w y* L; the part *w* is raiſed above the part *y* L by a bend or joint at *n*; the part *y* L being repreſented as fixed firm to the plate by the ſtrong ſcrew at *y*. This piece is called the potence, and is exactly ſimilar to the other ſupporting piece, which is called the cock, that is ſimilarly fixed to the oppoſite ſide of the plate and hid from the ſight in the figure. A little above the cylindrical pallet MSK (as it appears in the figure) is fixed a ſmall cylindrical piece of ſteel *in*, having a ſmall part projecting out at *i*, through which the verge alſo paſſes; this is called the lifting pallet; it fixes upon the verge like a collar, and is made faſt by a twiſt, ſo as to be ſet in any poſition with reſpect to the large pallet MSK. The balance lying below the plate PQRS, only the part UV is repreſented to view; the continuation of the poſition of the circumference, however, is repreſented by the dotted lines ULHV. The end EG of the long ſpring EF being made very ſlender, if a ſmall force be applied at the point *o* to preſs that end out from the wheel ABCD, it eaſily yields in that direction, turning as if were upon a center at G; it is alſo made to ſlide in a groove made in this ſtud in ſuch a manner that the end *o* may be placed at any required diſtance

Deſcription of the Eſcapement of Mr. Earnſhaw's time piece.

from

Defcription of the Efcapement of Mr. Earnfhaw's time piece.

from the center of the verge. Having defcribed the feveral parts as they appear in the figure, we next come to their connexion or fituation with refpect to each other. Let the long fpring EF be fuppofed to be fo placed that the end of the flender fpring *ri* may project a little way over the point of the lifting pallet *in*, but not fo clofe but that the point of the pallet may pafs by the hooked end of the fpring EF without touching it; the head of the adjufting fcrew *m* is alfo fuppofed to bear gently on the inner fide of the faid fpring EF, or that neareft to the wheel, and at the fame time the locking pallet is fo placed that one of the teeth D, of the balance-wheel, may juft take hold of it. This pallet is not vifible in its proper place in the figure, being covered from fight by the fcrew *m*, and part of the fpring EF; its pofition is therefore reprefented by the dot *k*, on the oppofite fide of the wheel, having the tooth A juft bearing up againft it. From the above defcription of the feveral parts of the efcapement, and their connexion with each other, it will be eafy to fee the mode of its action, which is as follows.

A force being fuppofed to be applied to the balance-wheel, fo as to caufe it to move round in the direction of the letters ABCD, one of the teeth, as D, will come up againft the locking pallet (as reprefented at A, and the locking pallet by *k*). The wheel is then faid to be locked, being prevented from moving forward by this pin. Let the balance be now fuppofed to reft in its quiefcent pofition, and it will have the fituation reprefented in the figure; the lifting point *i*, of the pallet *in*, will be juft clear of the projecting end of the flender fpring, the face *hl* of the large pallet MSK will fall a little below the point of the tooth B, and the balance having its fpiral or helical fpring applied to it (which is here fuppofed on the other fide of the plate PQRS, and of courfe not vifible in the figure) remains perfectly at reft in this pofition. Now as the balance ULHV, and the two pallets MSK and *in*, are fixed faft to the verge, it is plain they muft all move together; let therefore the balance be carried a little way round in the direction of the letters VULH; by this motion the end *i* of the lifting pallet *in* will be brought to prefs up againft the projecting end of the flender fpring, and as this fpring is fixed on the fide of the fpring EF, neareft to the balance-wheel, the point *i* will prefs the two fprings together out from the balance-wheel; then, as only the point of the tooth D (fee its pofition at *k*) touches the

locking

locking pallet, when the ſpring EF was at reſt again the head of the ſcrew *m*, it will, by the ſpring being preſſed out from the tooth, have ſlipped off (for the locking pallet which was before ſuppoſed at *k*, will now be at *a*, clear of the tooth A of the balance-wheel); the wheel being now at liberty will move round by the force ſuppoſed to be applied to it; but as the point *i* of the lifting pallet moves on and preſſes out the ſpring, the point *l* of the large pallet approaches towards the point of the tooth B of the balance-wheel, ſo that when the ſpring EF is ſufficiently puſhed out to unlock the wheel, the point *l* of the large pallet will be got to *d*, and in this poſition the point of the tooth B of the balance-wheel will fall upon it (ſee *Fig*. 2,) where the tooth B is repreſented in contact with the pallet at *l*; at the ſame time the point of the tooth D has juſt dropt off from the locking pallet *m*; the force of the wheel being by this means applied to the top of the pallet *hl*, gives an increaſed momentum to the balance, and aſſiſts it in its motion in the ſame direction, and by the continued motion of the large pallet in the direction MSK the point of the tooth B, which keeps preſſing and urging it forward, moves up towards the bottom of the face of the pallet towards *h*, until the plain flat ſurfaces of the tooth and pallet come into contact (ſee *Fig* 3); by this time the end *o* of the ſlender ſpring has dropt off from the point *i* of the lifting pallet, and the two ſprings have returned again into their quieſcent poſition, the ſpring EF gently bearing againſt the head of the adjuſting ſcrew *m*, and the locking pallet in a poſition to receive the next tooth C of the balance-wheel; (ſee the poſition of the point of the lifting pallet at *n* *Fig*. 3, alſo the locking pallet at *m*, and the approaching tooth at C.) When the two ſurfaces of the tooth and pallet are thus in contact, the greateſt force of the wheel is exerted upon the pallet, and of courſe upon the balance moving with it. The tooth ſtill preſſing againſt the face of the pallet, and the pallet moving in the direction MSK, it at laſt drops off, (ſee *Fig* 4, where *m* repreſents the poſition of the locking pallet, C the poſition of the tooth of the wheel juſt before it drops upon it, and *l h* the poſition of the face of the large pallet, having the point of the tooth B juſt ready to leave it at *l*,) leaving the balance at perfect liberty to move on in the ſame direction in which it was going. Juſt as the point of the tooth B, which has been preſſing the large pallet round, is ready to leave it, the

Deſcription of the Eſcapement of Mr. Earnſhaw's time piece.

Description of the Escapement of Mr. Earnshaw's time piece.

the next tooth C of the wheel is almost in contact with the locking pallet *m* (see *Fig.* 4) so that the instant the tooth B drops off the wheel is again locked, and the action of that tooth upon the balance is finished. As the balance moves with the greatest freedom upon its pivots, the force of the tooth has given it a considerable velocity, so that the balance still keeps moving on in the same direction, after the pressure of the tooth is removed by slipping off from the pallet, until the force of the pendulum spring (which is not represented in the figure) being continually increased by being wound up, overcomes the momentum of the balance, which, for an instant of time, is then stationary, but immediately returns by the action of the pendulum spring, which exerts a considerable force upon it in unwinding itself. As the balance returns, the point *i* of the lifting pallet *in* passes by the ends of the two springs EF and *yo*, and, in passing by, pushes the projecting end, *o*, of the slender spring in towards the balance-wheel, until it has passed it; which, as soon as it has done, the projecting end *o* again returns and applies itself close to the hooked end of the spring EF, as before. The spring *yo* is made so slender, that it gives but little resistance to the balance, during the time the point *i* of the lifting pallet is passing it, and of course causes but little (if any) decrease in its momentum. During the time the point *i* of the lifting pallet is passing the small spring *yo*, the long spring EF remains steadily bearing against the head of the adjusting screw *m*, as the hooked end at *o* just lets the end of the lifting pallet pass by without touching of it. As the spring has now been continually acting upon the balance, from the extremity of its vibration in the direction MSK, it has given it the greatest velocity, when the point *i* of the lifting pallet is passing the end *o* of the slender spring; for at this instant the spring which was wound up by the contrary direction of the balance, is now unwound again, or in the same state as it was in its quiescent position at first, and of course has no effect upon the balance at all in either direction; but the balance having now all the velocity it could acquire from the unwinding of the spring, goes on in the direction UVHL, until the force of this spring again stops it and brings it back again, moving in the same direction as at first, with a considerable velocity. By this return of the balance, the point *i* of the lifting pallet comes up again to the projecting end *o* of the slender spring,

pushes

pushes back the long spring EF, and unlocks the wheel; and another tooth falling upon the face of the pallet *hl* gives fresh energy to the balance: and thus the action is carried on as before.

The Escapement should be made in the following manner: The pivots of the balance axis should be the size of the verge-pivots of a good common sized pocket watch, and of the shape of *Fig.* 5. Pl. XIII, which will greatly add to their strength, the extreme end, or acting part only being straight; the jewel hole should be as shallow as possible, so as not to endanger cutting the pivot, and the part of action of the hole made quite back with only a very shallow chamfer behind to retain the oil; deep holes are very bad, for when the oil becomes glutinous, it will make the pivots stick so as to prevent the balance from its usual vibration. The pallet should be half the diameter of the wheel, or a little larger, for if smaller, the wheel will then have too much action on it, which will increase friction most considerably, and likewise cause the balance to swing so much farther to clear the wheel; consequently a check in the motion of the balance may stop the watch. The face of the pallet should run in a line of equal distance between the centre of the pallet and its extremity, and not in a right line to its centre, that is an increase of friction, and a loss of that power which is obtained by the wheel acting on the extremity of the pallet: this is clearly proved by time, by the hole worn by the points of teeth in all pallets that run in a line to the centre. The scape wheel teeth should form the same direction as the face of the pallet, under cut for the said purpose of avoiding friction, and maintaining the power, and for safe locking. The points of the wheel teeth must not be rounded off, but left as sharp as possible. The pivots of the scape wheel are to be a very little larger than the balance pivots.

Instructions for making the Escapement.

The wheel is locked by a spring instead of a detent with pivots, as the French have made them, for those pivots must have oil, and when the oil thickens then the spring of the pivot detents is so affected by it as to prevent the detent from falling into the wheel quick enough, the consequence of this is irregular time and stoppage of the watch, and if ever such a watch went well for twelve months, chance must have had by far the most hand in it.

Detent with a spring joint.

When the spring is planted on the side of the wheel, as in my escapements, the part on which the wheel rests should be a little

How to place the detent, pallets, &c.

a little ſhort of a right angle, ſo that the wheel may have a tendency to draw the ſpring into it, for if ſtopped the other way, or beyond a right angle it will have a tendency to puſh the ſpring out; in that caſe the wheel will have liberty to run: the wheel ſhould take no more hold on the ſpring than juſt ſufficient to ſtop it, for if more, friction will be increaſed. The ſmall return ſpring ſhould be as thin as poſſible at the end faſtened to the other ſpring, but at the outer end a little thicker; the ſpring ſhould be planted down as cloſe to the wheel as to be juſt free of it. The diſcharging pallet about one-third, or near one-half the ſize of the large or main pallet, the face of it in a right line to the centre, the back of it a little rounding and off from the centre. Great care muſt be uſed in taking off the edges of this diſcharging piece, to make it round to prevent cutting the ſpring, nor can it be made too thin ſo it does not cut; the end of it neareſt the ballance ſhould be a little more out from the centre of the ballance axis than the lower part of it towards the potence, for counteracting the natural tendency of the ſpring downwards from the preſſure of the ſcape wheel; and that part of the ſpring on which the wheel reſts ſhould be ſtopped a little down to give the wheel a tendency to force it up, to counteract the natural inclination the wheel has to draw it down by its preſſure on it.

Conſtruction of the balance with its compenſation weights, &c.

The balance is to be made of the beſt ſteel, and turned from its own centre to its proper ſize, then put it into a crucible with as much of the beſt braſs as when melted will cover it. The braſs melted will adhere to the ſteel (for if any other metal is uſed by way of ſolder, that watch cannot go well), then turn it to its proper thickneſs, and hollow it out ſo as to leave the ſteel rim about the thickneſs of a repeating ſpring to a ſmall ſized repeating watch, turn the braſs to twice or near three times that thickneſs of ſteel, croſs it out with only one arm ſtraight acroſs the centre, and at each end of the arm fix two ſcrews oppoſite to each other through the rim of the balance to regulate the watch to time, the diameter of the heads of theſe ſcrews about equal to the thickneſs of the balance, a little more or leſs is not material. The compenſation weights ſhould be made of the beſt braſs and well hammered, and a groove turned to let the rim of balance into it, and this ſhould be cut into fourteen equal parts by a wheel engine,

gine, then you will have seven pair of pieces of equal size and weight; two of these pieces being screwed on the rim of the balance at equal distances will produce an equilibrium, a balance in the full sense of the word, equal in all its parts. In making balances great care should be taken that they get no bruises or bendings, for if they get a bruise on one side so as to indent the metal, that part will be less affected by heat and cold than the other parts which have not received the same violence to close its pores.

Adjustment to keep time in all temperatures,

To adjust the balance in heat and cold—put the watch into about 85 or 90 degrees of heat, by the common thermometer, mark down exactly how much it gains or loses in 12 hours, then put it into as severe cold as you can get for 12 hours, and if it gains one minute more in 12 hours in cold than in heat, move the compensation weights farther from the arm of the balance about $\frac{1}{8}$ of inch, and if it gains one minute more in 12 hours in heat than in cold move the weights $\frac{1}{8}$ of inch nearer to the arm of the balance, and so on in the like proportion, trying it again and again till you find the watch go the same in whatever change of heat or cold you put it.

—and in all positions,

Much difficulty has fallen to the lot of watchmakers in the endeavour to make timekeepers go nearly the same in the different positions. I have had my share of this, but it is now over; by far the greatest part of this difficulty arises from the balance spring not being properly made. But if the spring is made, as I shall describe hereafter, you have only to make the balance of equal weight and it will go within a few seconds per day in all positions alike, and if it vibrates not more than one circle and a $\frac{1}{4}$, by applying a small matter of weight to that part of the balance which is downward when in the position that it loses most, will correct it with great accuracy; but if it vibrates more than one circle and a $\frac{1}{4}$, then it will require the weight to be above instead of below; and after the watch has been going a few months and its vibration shortens to one $\frac{1}{4}$ circle, then it will go worse and worse by reason of the weight being in the wrong place; therefore, to avoid this evil, it is absolutely necessary to confine the vibrations to one $\frac{1}{4}$ circle, which will produce the most steady performance. It is common for watchmakers to adore a timekeeper when they see it vibrate a circle and a half, or more, and form an opinion of its excellence from this only; but I know

Rule. If the balance be in poise it will go nearly alike in all positions. Correct it by adding weight to the lowest part when in losing position if the same vibration be about 200°; if more degrees then add to the upper part. Hence a moderate vibration is best.

know from experience what would be the consequence, and have been condemned, because, when I have seen such watches I said I saw enough to declare that it would not give very accurate performance.

Concerning the balance spring.

Balance spring. To find out the invisible properties of this apparent simple part of the machine, has given much more trouble than all the rest. I despaired of bringing timekeepers to the state I have done, and unless those hidden properties are known to timekeeper makers, however well they may execute all other parts they will find their most sanguine expectations frustrated. I have seen watchmakers boast of their timekeepers going well for a month or two, and from the knowledge I had of the effects produced by the balance spring, I have told them that a month or two more would destroy their hopes. The cylindrical spring being in all its turns of equal distance from the centre, in course every turn will be of equal strength, and called isochronal, and believed that all vibrations whether long or short would be performed in the same time; but this is not true, for if a man is to go four miles in the same time as he has gone one mile, he cannot do it with the same power; no, he must have impelling force to quicken his motion, or he will be four times as long in doing it. Therefore instead of the spring being equal in all its parts, it must be made to increase in thickness to the outer end, in such proportion as will cause the balance when thrown to a greater distance to return so much the quicker to make them equal; by long perseverance I found how to make such springs, and then I thought I had got all I wished for. But cruel disappointment nearly broke my heart, for I found I had yet another difficulty to break down, as my watches with such perfect springs were continually losing on their rates. What farther to do I knew not, and I own I was nearly if not quite mad. But obstinate in the cause and resolving not to give it up but with life, perseverance came once more to my aid—and with still more unremitting study, which nearly finished me, before I applied the following remedy for the before mentioned evil, I found, in the course of reasoning on bodies, that watch springs relax and tire like the human frame, when kept constantly in motion, and this may be proved by the following experiment: let a watch that has been going a few months go down, let it be down for a week

It is made tapering.

Springs are subject to a relaxation of force [illegible] is regained by rest.

or two, or more, then set it going, and if it be a good timekeeper so as not to be affected by the weather it will go some seconds per day faster than it did when it was let down, but it will again lose its quickness in a gradual manner gaining less and less till it comes to its former rate. Therefore finding that isochronal springs would not do—and likewise having made springs of such shape as would render long and short vibrations equal in time—constantly lose the longer the watch went, I then made them of such shape as to gain in the short vibrations about five or six seconds per day more than the long ones, this quantity could only be found by long experience, and the way I proved this was to try the rate of the watch with the balance vibrating about ¼ of circle, then tried its rate vibrating one circle and a ¼, and if the short vibrations go slower than the long ones that watch will lose on its rate, and if they are equal, it will likewise lose, but that only from relaxation, and if it gains in the short vibrations more than five or six seconds in twenty-four hours it will in the long run gain on its rate, but if not more than that quantity, and the timekeeper is perfect in heat and cold and every other part, the above properties will render it deserving of the name of a perfect timekeeper, and this is a principal cause of my timekeepers excelling all others, and this the principal cause of some of my timekeepers going better than others, though made by me, the springs of them being made to accord more exactly to the above proportions; and this is the cause which has enabled me to foretel what my timekeepers would do, which Dr. Maskelyne, Mr. Crosley, and others can testify. The above effect is produced as follows. I find the common relaxation of balance springs to be about five or six seconds per day on their rates in the course of a year, therefore if the short vibrations are made by the shape of the spring to go about that quantity faster than the long ones, and as the spring relaxes in going by time so the watch accumulates in dirt and thickening of the oil which shortens the vibrations, the short ones then being quicker, compensates for the evil of relaxation of the balance spring. From this it is plain, that the causes of error in timekeepers are not undefined and vague in their nature, which has been supposed; for when it is certain that all causes of error may be over compensated we cannot despair of finding the medium, and which

This gradual effect causes a loss on the rate which may be compensated by giving greater speed to the shorter vibrations in the first construction.

which may be easily proved by examining the going of my timekeepers. It will there appear that what errors, they are subject to, arise from causes certain and natural, and in course may be corrected by art*.

IX.

Experimental Enquiry into the Proportion of the several Gases or Elastic Fluids, constituting the Atmosphere. By JOHN DALTON.†

On the component parts of the atmosphere.

IN a former paper which I submitted to this society, "on the constitution of mixed gases," I adopted such proportions of the simple elastic fluids to constitute the atmosphere as were then current, not intending to warrant the accuracy of them all, as stated in the said paper; my principal object in that essay was, to point out the *manner* in which mixed elastic fluids exist together, and to insist upon what I think a very important and fundamental position in the doctrine of such fluids:—namely, that the elastic or repulsive power of each particle is confined to those of its own kind; and consequently the force of such fluid, retained in a given vessel, or gravitating, is the same in a separate as in a mixed state, depending upon its proper density and temperature. This principle accords with all experience, and I have no doubt will soon be perceived and acknowledged by chemists and philosophers in general; and its application will elucidate a variety of facts, which are otherwise involved in obscurity.

Objects of this essay.

The objects of the present essay are,

1. To determine the weight of each sep. atmosphere.

1. To determine the weight of each simple atmosphere, abstractedly; or, in other words, what part of the weight o the whole compound atmosphere is due to azote; what to oxygen, &c. &c.

—and the relative weights of the gases at the surface of the earth

2. To determine the relative weights of the different gases in a given volume of atmospheric air, such as it is at the earth's surface.

* To this communication Mr. Earnshaw has annexed two plates with descriptions, shewing the parts of his time-piece; all which, except those of the Escapement (which we have given) are capable of the same variations as those of any other good movements. He asserts that the best train for time keepers is 18,000; that the scape wheel for pocket ones should have 15 teeth, and for box ones 13 teeth.

† Manchester Mem. Vol. V.

3. To investigate the proportions of the gases to each other, such as they ought to be found at different elevations above the earth's surface.

—as well as at different elevations.

To those who consider the atmosphere as a chemical compound, these *three* objects are but *one*; others, who adopt my hypothesis, will see they are essentially distinct. With respect to the first: It is obvious, that, on my hypothesis, the density and elastic force of each gas at the earth's surface, are the effects of the weight of the atmosphere of that gas solely, the different atmospheres not gravitating one upon another. Whence the first object will be obtained by ascertaining what share of elastic force is due to each gas in a given volume of the compound atmosphere; or, which amounts to the same thing, by finding how much the given volume is diminished under a constant pressure, by the abstraction of each of its ingredients singly. Thus, if it should appear that by extracting the oxygenous gas from any mass of atmospheric air, the whole was diminished $\frac{1}{5}$ in bulk, still being subject to a pressure of 30 inches of mercury; then it ought to be inferred that the oxygenous atmosphere presses the earth with a force of six inches of mercury, &c.

Now each single atmosphere presses by its whole weight; which is measured by its spring and that by its volume.

Take away one of the gases and the loss of volume represents its pressure and the weight of that atmosphere.

In order to ascertain the second point, it will be further necessary to obtain the specific gravity of each gas; that is, the relative weights of a given volume of each in a pure state, subject to the same pressure and temperature. For the weight of each gas in any given portion of atmospheric air, must be in the compound ratio of its force and specific gravity.

The weights of each gas in given volume had from sp. gravity.

With respect to the third object, it may be observed, that those gases which are specifically the heaviest must decrease in density the quickest in ascending. If the earth's atmosphere had been a homogeneous elastic fluid of the same weight it is, but ten times the specific gravity, it might easily be demonstrated that no sensible portion of it could have arisen to the summits of the highest mountains. On the other hand, an atmosphere of hydrogenous gas of the same weight, would support a column of mercury nearly 29 inches on the summit of Mount Blanc.

The proportions at different heights are obtained from the progression with each gas in the same manner as in computation for the whole atmosphere.

The several gases constantly found in every portion of atmospheric air, and in such quantities as are capable of being appreciated, are azotic, oxygenous, aqueous vapour, and carbonic acid. It is probable that hydrogenous gas also is constantly

ſtantly preſent; but in ſo ſmall proportion as not to be detected by any teſt we are acquainted with; it muſt therefore be confounded in the large maſs of azotic gas.

1. *On the weight of the Oxygenous and Azotic Atmoſpheres.*

Proceſſes for determining the oxigen in the atmoſphere.

Various proceſſes have been uſed to determine the quantity of oxygenous gas.

1. with nitrous gas

1. The mixture of nitrous gas and air over water.

2. with ſulphuret

2. Expoſing the air to liquid ſulphuret of potaſh or lime, with or without agitation.

3. Exploſion with hidrogen.

3. Exploding hydrogen gas and air by electricity.

4. Expoſure to green ſulphat of iron.

4. Expoſing the air to a ſolution of green ſulphat or muriat of iron in water, ſtrongly impregnated with nitrous gas.

5. Burning phoſphorus.

5. Burning phoſphorus in the air.

All produce the ſame reſult.

In all theſe caſes the oxygen enters into combination and loſes its elaſticity; and if the ſeveral proceſſes be conducted ſkilfully, the reſults are preciſely the ſame from all. In all parts of the earth and at every ſeaſon of the year, the bulk of any given quantity of atmoſpheric air appears to be reduced nearly 21 per cent. by abſtracting its oxygen. This fact, indeed, has not been generally admitted till lately; ſome chemiſts having found, as they apprehended, a great difference in the quantity of oxygen in the air at different times and places; on ſome occaſions 20 per cent. and on others 30, and more of oxygen are ſaid to have been found. This I have no doubt was owing to their not underſtanding the nature of the operation and of the circumſtances influencing it. Indeed it is difficult to ſee, on any hypotheſis, how a diſproportion of theſe two elements ſhould ever ſubſiſt in the atmoſphere.

The oxigen and azote are not [illegible]ible.

The firſt proceſs with nitrous gas tho' diſcredited, is here perfected.

As the firſt of the proceſſes above-mentioned has been much diſcredited by late authors, and as it appears from my experience to be not only the moſt elegant and expeditious of all the methods hitherto uſed, but alſo as correct as any of them, when properly conducted, I ſhall, on this occaſion, animadvert upon it.

Inſtructions for the proceſs.

Pure nitrous gas

1. Nitrous gas may be obtained pure by nitric acid diluted with an equal bulk of water poured upon copper or mercury; little or no artificial heat ſhould be applied. The laſt product of gas thus obtained, does not contain any ſenſible portion of azotic gas: at leaſt it may eaſily be got with leſs than two or three *per cent.* of that gas: It is probably nearly free from nitrous oxide alſo, when thus obtained.

2. If 100 measures of common air be put to 36 of pure nitrous gas in a tube 3-10th of an inch wide and 5 inches long, after a few minutes the whole will be reduced to 79 or 80 measures, and exhibit no signs of either oxygenous or nitrous gas.

Mixture 100 air and 36 n. gas in a narrow tube. Residue about 80 azote.

3. If 100 measures of common air be admitted to 72 of nitrous gas in a wide vessel over water, such as to form a thin stratum of air, and an immediate momentary agitation be used, there will, as before, be found 79 or 80 measures of pure azotic gas for a residuum.

Mixture 100 air and 72 n. gas in a wide vessel with agitation. Residue as before 80 azote.

4. If, in the last experiment, *less* than 72 measures of nitrous gas be used, there will be a residuum containing oxygenous gas; if *more*, then some residuary nitrous gas will be found.

Intermediate proportion of n. gas leaves either n. gas or oxigen with the azote.

These facts clearly point out the theory of the process: the elements of oxygen may combine with a certain portion of nitrous gas, or with twice that portion, but with no intermediate quantity. In the former case *nitric* acid is the result; in the latter *nitrous* acid: but as both these may be formed at the same time, one part of the oxygen going to *one* of nitrous gas, and another to *two*, the quantity of nitrous gas absorbed should be variable; from 36 to 72 *per cent.* for common air. This is the principal cause of that diversity which has so much appeared in the results of chemists on this subject. In fact, all the gradation in quantity of nitrous gas from 36 to 72 may actually be observed with atmospheric air of the same purity; the wider the tube or vessel the mixture is made in, the quicker the combination is effected, and the more exposed to water, the greater is the quantity of *nitrous* acid and the less of nitric that is formed.

Theory of the process. In the first case nitric gas was formed; in the latter nitrous.

To use nitrous gas for the purpose of eudiometry therefore, we must attempt to form *nitric acid* or *nitrous* wholly, and without a mixture of the other. Of these the former appears from my experiments to be most easily and most accurately effected. In order to this a narrow tube is necessary; one that is just wide enough to let air pass water without requiring the tube to be agitated, is best. Let little more nitrous gas than is sufficient to form nitric acid be admitted to the oxygenous gas; let no agitation be used; and as soon as the diminution appears to be over for a moment let the residuary gas be transferred to another tube, and it will remain without any further diminution of consequence. Then $\frac{7}{19}$ of the loss will be due

Practical remark. Operate so as to form the nitric gas.

to oxygen. The transferring is necessary to prevent the nitric acid formed and combined with the water, from absorbing the remainder of the nitrous gas to form nitrous acid.

Method with sulphuret.

Sulphuret of lime is a good test of the proportion of oxygen in a given mixture, provided the liquid be not more than 20 or 30 per cent. for the gas (atmospheric air); if the liquid exceed this, there is a portion of azotic gas imbibed somewhat uncertain in quantity.

Volta's method.

Volta's eudiometer is very accurate as well as elegant and expeditious: according to Monge, 100 oxygen require 196 measures of hydrogen; according to Davy 192; but from the most attentive observations of my own, 185 are sufficient. In atmospheric air I always find 60 per cent. diminution when fired with an excess of hydrogen; that is, 100 common air with 60 hydrogen, become 100 after the explosion, and no oxygen is found in the residuum; here 21 oxygen take 39 hydrogen.

2. *Of the Weight of the Aqueous Vapour Atmosphere.*

To find the weight of aqueous vapour in the atmosphere.

I have, in a former essay, (Manchester Mem. vol. 5. p. 2, page 559.) given a table of the force of vapour in *vacuo* for every degree of temperature, determined by experiment; and in the sequel of the essay, have shewn that the force of vapour in the atmosphere is the very same as in *vacuo*, when they are both at their utmost for any given temperature. To find the force of aqueous vapour in the atmosphere, therefore, we have nothing more to do than to find that degree of cold at which it begins to be condensed, and opposite to it in the table abovementioned, will be found the force of vapour. From the various facts mentioned in the essay it is obvious, that vapour contracts no chemical union with any of the gases in the atmosphere; this fact has since been enforced in the Annales de Chimie, vol. xlii. by Clement and Desorme.

M. De Saussure found by an excellent experiment, that dry air of 64° will admit so much vapour as to increase its elasticity, $\frac{1}{54}$. This I have repeated nearly in his manner, and found a similar result. But the table he has given us of aqueous vapour at other temperatures is very far wrong, especially at temperatures distant from 64°. The numbers were not the result of direct experiment, like the one above. If we could obtain the temperatures of all parts of the earth's surface,

furface, for any given time, a mean of them would probably be 57° or 58°. Now if we may fuppofe the force of vapour equivalent to that of 55°, at a medium, it will, from the table, be = to .443 of mercury; or, nearly $\frac{1}{70}$ of the whole atmofphere. This it will be perceived is calculated to be the weight of vapour in the whole atmofphere of the earth. If that incumbent over any place at any time be required, it may be found as directed above.

It is on an average about one seventieth.

3. *Of the Weight of the Carbonic Acid Atmofphere.*

From fome obfervations of Humboldt, I was led to expect about $\frac{1}{100}$ part of the weight of the atmofphere to be carbonic acid gas: but I foon found that the proportion was immenfely over-rated. From repeated experiments, all nearly agreeing in their refults, and made at different feafons of the year, I have found, that if a glafs veffel filled with 102,400 grains of rain water be emptied in the open air, and 125 grains of ftrong lime water be poured in, and the mouth then clofed; by fufficient time and agitation, the whole of the lime water is juft faturated by the acid gas it finds in that volume of air. But 125 grains of the lime water ufed require 70 grain meafures of carbonic acid gas to faturate it: therefore, the 102,400 grain meafures of common air contain 70 of carbonic acid; or $\frac{1}{1460}$ of the whole. The weight of the carbonic acid atmofphere then is to that of the whole compound as 1:1460; but the weight of carbonic acid gas in a given portion of air at the earth's furface, is nearly $\frac{1}{1000}$ of the whole; becaufe the fpecific gravity of the gas is $1\frac{1}{2}$ that of common air. I have fince found that the air in an affembly, in which two hundred people had breathed for two hours, with the windows and doors fhut, contained little more than 1 per cent. of carbonic acid gas.

Deduction of the weight of carbonic acid in the atmofphere about one thoufandth.

Having now determined the force with which each atmofphere preffes on the earth's furface, or in other words, its weight; it remains next to enquire into their fpecific gravities.

Specific gravities of gases.

These may be seen in the following Table.

Atmospheric air	1.000
Azotic gaz	.966
Oxygenous gas	1.127
Carbonic acid gas	1.500
Aqueous vapour	.700
Hydrogenous gas	.077*

Kirwan and Lavoisier are my authorities for these numbers; except oxygenous gas and aqueous vapour. For the former I am indebted to Mr. Davy's Chemical Researches; his number is something greater than theirs: I prefer it, because, being determined with at least equal attention to accuracy with the others, it has this further claim for credit, that 21 parts of gas of this specific gravity, mixed with 79 parts of azotic gas, make a compound of exactly the same specific gravity as the atmosphere, as they evidently ought to do, setting aside the unfounded notion of their forming a *chemical* compound. The specific gravity of aqueous vapour I have determined myself both by analytic and synthetic methods, after the manner of De Saussure; that is, by abstracting aqueous vapour of a known force from a given quantity of air, and weighing the water obtained—and admitting a given weight of water to dry air and comparing the loss with the increased elasticity. De Saussure makes the specific gravity to be ,71 or ,75; but he used caustic alkali as the absorbent, which would extract the carbonic acid as well as the aqueous vapour from the air. From the experiments of Pictet and Watt, I deduce the specific gravity of aqueous vapour to be ,61 and ,67 respectively. Upon the whole, therefore, it is probable that ,7 is very nearly accurate.

We have now sufficient data to form tables answering to the two first objects of our enquiry.

* The specific gravity of hydrogen must be r[illegible] too low: if 100 oxygen require 185 hydrogen by measure, according to this 89 oxygen would require only 11 hydrogen to form water; whereas 85 require 15. Hydrogen ought to be found about $\frac{1}{10}$ part of the weight of common air.

I. *Table of the Weights of the different Gases constituting the Atmosphere.*

	Inch of Mercury.
Azotic gas - - - - -	23.36
Oxygenous gas - - - -	6.18
Aqueous vapour - - - -	.44
*Carbonic acid gas - - - -	.02
	30.00

Absolute weights of the different gases in the whole atmosphere.

II. *Table of the proportional Weights of the different Gases in a given Volume of Atmospheric Air, taken at the Surface of the Earth.*

	per cent.
Azotic gas - - - - -	75.55
Oxygenous gas - - - -	23.32
Aqueous vapour - - - -	1.03*
Carbonic acid gas - - - -	—.10
	100.00

Weights of the different gases in equal bulks at the earth's surface.

III. *On the Proportion of Gases at different Elevations.*

Computation of the proportion of gases above the earth's surface. It is not notably different at any accessible height.

M. Berthollet seems to think that the lower strata of the atmosphere ought to contain more oxygen than the upper, because of the greater specific gravity of oxygenous gas, and the slight affinity of the two gases for each other. (See Annal. de Chimie, Tom. 34. page 85.) As I am unable to conceive even the possibility of two gases being held together by affinity, unless their particles unite so as to form *one* centre of repulsion out of two or more (in which case they become *one* gas) I cannot see why rarefaction should either decrease or increase this supposed affinity. I have little doubt, however, as to the fact of oxygenous gas observing a diminishing ratio in ascending; for, the atmospheres being independent on each other, their densities at different heights must be regulated by their specific gravities. Hence, if we take the azotic atmo-

* The proportion of aqueous vapour must be understood to be variable for any one place: the others are permanent or nearly so.

ſphere as a ſtandard, the oxygenous and the carbonic acid will obſerve a decreaſing ratio to it in aſcending, and the aqueous vapour an increaſing one. The ſpecific gravity of oxygenous and azotic gaſes being as ſeven to ſix nearly, their diminution in denſity will be the ſame at heights reciprocally as their ſpecific gravities. Hence it would be found, that at the height of Mount Blanc (nearly three Engliſh miles) the ratio of oxygenous gas to azotic in a given volume of air, would be nearly as 20 to 80;—conſequently it follows that at any ordinary heights the difference in the proportions will be ſcarcely if it all perceptible *.

X.

Obſervation which indicates a ſpontaneous Decompoſition of nitrous Acid and Formation of Ammonia. By D. A.

To Mr. NICHOLSON.

SIR,

Decompoſition of nitrous acid.

I SEND you a ſtatement of the following fact, in caſe it may not hitherto have been obſerved: it ſeems to ſhew the mutual decompoſition of nitrous acid and atmoſpheric air; but the explanation of the theory I will leave to you, or ſome of your learned correſpondents. A phial of bright orange coloured nitrous acid, ſo looſely ſtopped that bubbles of gas eſcaped every five or ten minutes, having ſtood within a few inches of a bottle of muriatic acid, cloſely ſtopped for above a twelvemonth, my attention was attracted by obſerving a white incruſtation of ſalts upon the label paper of the laſt mentioned phial. To determine their nature, diſſolved them in diſtilled water; dropped a little nitric acid in, to ſaturate any uncombined alcalies; then with nitrate of ſilver, a copious precipitate was formed, which indicates the muriatic to be the acid: when I ſaturated the acids with pure potaſs, the ſmell

* Air brought from the ſummit of Helvelyn, in Cumberland (1100 yards above the ſea—Barometer being 26,60) in July 1804, gave no perceptible difference from the air taken in Mancheſter.—M. Gay-Luſſac determines the conſtitution of air brought from an elevation of four miles to be the ſame as that at the earth's ſurface.

fmell refembled ammonia; but owing to the folution being fo extremely weak, was fcarce perceptible; but on a finger being dipped into it, and held near a ftopper, moiftened with muriatic acid, evidently produced a white cloud, which difappeared upon withdrawing the acid, and re-appeared on its approach; which teft alone I think may be fufficient to prove ammonia to have been the bafe. I may obferve, that the falts were formed only on that part of the table on which fome muriatic acid had been fpilt; the neck of the nitrous acid phial was covered with a moifture, which had a confiderable ammoniacal fmell, and exhibited the fame appearances with the moiftened ftopper, and was therefore uncombined ammonia, and feems to fhew that the prefence of the muriatic acid was not neceffary for its formation. I have endeavoured to be as concife as poffible, and remain

Your conftant reader,

April 17, 1805. D. A.

SCIENTIFIC NEWS.

*Note on the Porcelain of Reaumur Communicated by Veau de Launai**.

M. PECARD of Tours, manufacturer of Rouen ftone ware, has repeateded in his furnace Reaumur's experiment of transforming glafs into porcelain; mentioned in the memoir of the Academy of Sciences, for the year 1739, p. 370. M. Pecard obtained a devitrification as complete within as without. His experiment was made upon a common glafs bottle from the Ancenis Foundry. The bottle was filled with Nevers fand, and depofited in a *fagger*, which was afterwards filled up with the fame fort of fand. The fagger or cafe was placed with others, containing earthenware in the chimney or upper part of the furnace, and heated as ufual. When the operation was finifhed, and the furnace was fufficiently cooled, the bottle was taken from its bed of fand in the fagger, and emptied of its contents. The bottle had undergone no alteration of fhape; but its green colour and tranfparency were exchanged for milky opacity, equally fpread over all parts of the bottle. In this, his firft experiment, Reaumur's porcelain made in an improved ftate.

* Journal de Phifique, Vol. LXI. p. 401.

periment, M. Pecard has obtained a much more equal devitrification than that procured by Reaumur; who remarks in his memoir, that he thinks it not impossible that this point of equality between the internal and external parts may be obtained.

This substance is much harder than glass; it readily gives a spark with steel; and from the advantages it seems to hold forth in many respects deserves to be made an object of investigation.

Darcet applies the glass porcelain to useful purposes.

A distinguished chemist, who pursues the steps of his father, whose name will be ever dear to the sciences, and to those who cultivate them, M. Darcet, has already made several experiments on this interesting subject, which form part of a work not yet completed. He has made mullers of this substance, exceeding the hardness of flint; also capsules and other articles which easily support the fire, and are not subject to the power of re-agents, such as sulphuric acid, &c. The little cost of the materials whereof these vessels, &c. are fabricated, induce a hope that the labours of Reaumur on this subject will be resumed, and carried on in a way that will be of utility in different arts.

Anatomical Work.

AN extensive work on the anatomy of the organs of hearing in animals, generally, together with the physiology of their several parts, and a series of accoustic phenoména intended to elucidate the subject, is in forwardness for publication this spring by Anthony Carlisle, F. R. S. F. L. S. and surgeon to the Westminster Hospital.

Fig 1

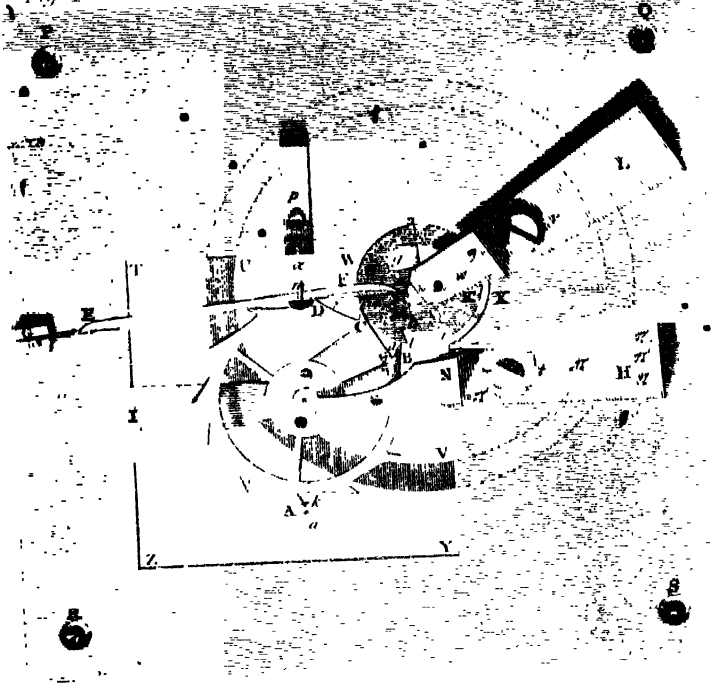


Fig 2

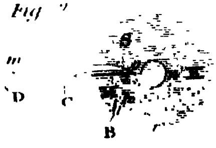


Fig 3

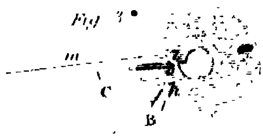
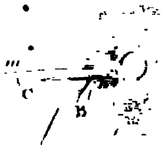


Fig 4



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THE END OF THE THIRTEENTH VOLUME.

Printed by W. Stratford, Crown-Court, Temple-Bar.